THE

POPULAR SCIENCE REVIEW.

A QUARTERLY MISCELLANY OF ENTERTAINING AND INSTRUCTIVE ARTICLES ON SCIENTIFIC SUBJECTS.

EDITED BY W. S. DALLAS, F.L.S.
ASSISTANT-SECRETARY OF THE GEOLOGICAL SOCIETY

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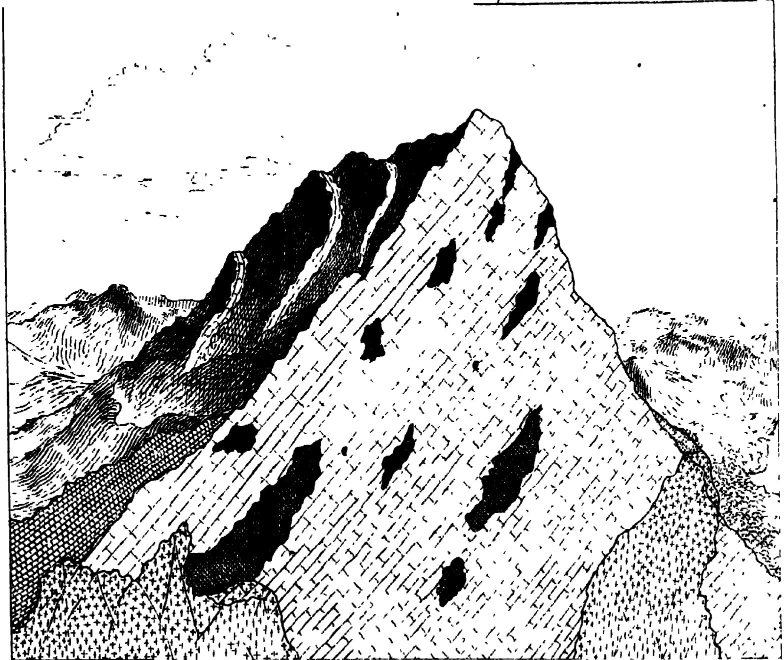
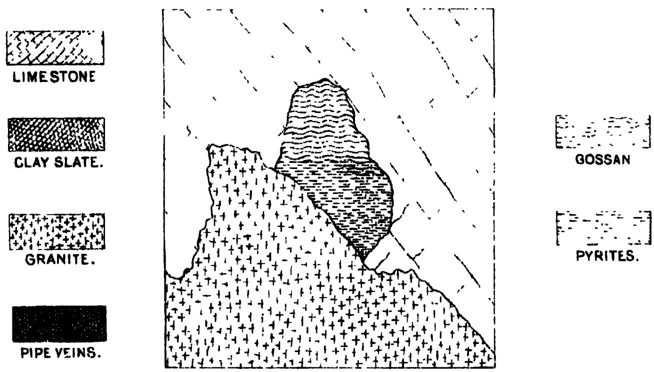


Fig. 1.



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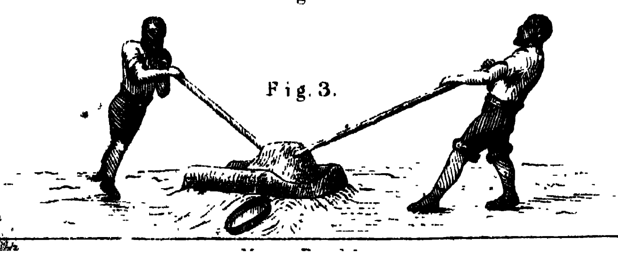

Fig. 3.

POPULAR SCIENCE REVIEW.

NOTES ON THE ARGENTINE REPUBLIC.

By CHARLES OXLAND.

[Plate I.]

THE immense territory possessed by the Argentine Republic, about 1,152,000 square miles, with a population of only 1,200,000 inhabitants, forcibly recalls a remarkable feature in ancient Spanish colonization. The Spaniards in their expeditions were usually not contented with safely establishing one point only on the sea-coast, but they pressed forward to the interior, making new conquests, and often leaving behind them small garrisons which the Indians required but little provocation to attack. This system frequently led to serious disasters, as is instanced by their chief seaport settlement of Buenos Ayres, which, on account of persistent Indian attacks, was twice abandoned, previous to its final settlement in 1580. The courage of the Spanish settlers was sorely tried, and the history of the country shows that the wide extent over which the Indians have ranged, from the 'Gran Chaco,' or Indian territory of the north, to Patagonia in the south, has been one of the principal causes of the slow development of the interior provinces along the eastern slope of the Andes.

Between Buenos Ayres and Rosario a short distance is now travelled by rail, whence steamers take passengers up the Parana. The variety of scenery and the richness of vegetation on this river make this journey a great treat to the lover of nature; the beautiful bits of tree, shrub, and bank, that open up full of colour and graceful form at each successive bend are almost bewildering, and make one long to have time to absorb the whole scene. In many places the banks of the river are flanked by higher ground, which shows signs of having, at some time, bounded a stream compared to which the present one would be a pigmy in size. The Parana, as it now is, allows steamers of 1500 to 2000 tons to go up to Rosario, a distance of nearly two hundred miles; and navigation for small

craft is possible to a distance of one thousand miles from its mouth.

Rosario, the terminus of the railroad, built immediately on the banks of the river, is only second in importance to Buenos Ayres as a centre for the receipt of the produce of the country intended for export, as also for the distribution of imports. From here the railroad strikes out across the flat country of the Pampas to Villa Maria, from which place one branch runs northwards to Cordova, 245 miles from Rosario, and to Tucuman, 340 miles from that city, and the other towards the south-west as far as Villa de Mercedes, about 190 miles, from which point it is proposed soon to extend it to Mendoza, San Juan, and over the Andes to Chili.

In the run to Villa de Mercedes as well as to Cordova, the rail passes over Pampas largely utilized for grazing cattle; the soil is alluvial, and gravelly in patches, but mainly the former, the alluvium being from two to eight feet in depth, under which lie beds of detritus of great thickness, often more than eighty feet.

Farms are spread out across the country, mainly employed in raising horned cattle, horses and sheep, of which late estimates give about seventeen to eighteen millions of horned cattle, and ninety to ninety-five millions of sheep; the land offers good opportunities in this direction for enterprising and determined men, but great energy is required to manage well during seasons of drought.

The settlers along the line of rail, among whom are included English, Scotch, Irish, French, Germans, and Italians, look healthy, and as though they enjoyed a good climate.

The ride by rail is not particularly interesting, excepting in the opportunity it affords of seeing a great variety of human nature, and of accumulating a weariness of the flatness of the country, which makes one long for a sight of the Sierras, or of the Cordillera. One thing is very striking, namely, that as you *pass up the line of rail towards Villa de Mercedes*, the number of foreigners becomes gradually less, until at the above terminus they are almost wholly left behind. This may be accounted for from the greater risks incurred in farming in these parts from occasional raids by bands of Indians; the line of military outposts extended across the country to San Rafael, in the province of Mendoza, being here closer to the road.

At Villa de Mercedes, the present terminus of the railroad, the scene changes to one of greater interest, with more numerous chances of adventure. From here coaches ply twice a-week to the mountain provinces. The paper-money of the provinces, off the lines of railroad, is represented by the Bolivian silver half and quarter dollar pieces. This is the

case also with the 'Fuerte,' or enforced national paper, which is not current away from the provincial centres and larger towns.

Villa de Mercedes is a little to the west and south of the Sierra de Cordova, a range chiefly made up of granitic rocks. Travellers here take a conveyance less comfortable than a Californian stage-coach, and more like a light but strong omnibus. The small quantity of luggage allowed free of charge—twenty-five pounds—is packed on the top. Two shaft mules with driver, two other pairs with a gaucho postilion for each, with conductor, seven people inside and three behind the driver, on a sort of box-seat, make the complement. The scenes that occur at a start are often very amusing; the disputes over seats, the anxiety of passengers about their packages and edibles for the road, but more often the capers of half-wild mules when being harnessed. The writer saw one of these animals throw itself completely backwards three times, making it necessary on each occasion to re-harness it, and after the third attempt another was put in its place.

All being ready, the driver, a wiry half-breed, gathers up his reins and cracks his whip, the gauchos tighten the traces, ply their spurs, and away goes the coach at a wild pace, swaying and creaking down the sandy road to the open country, with the conductor executing flourishes on his bugle. The dust and heat detract but little from the freshness of such a scene to an European, especially as the road passes towards rising ground.

After a distance of ten or twelve miles, the bugle sounds long and clearly as we cross some rolling country with occasional trees and shrubs. Suddenly, two men on horseback are seen driving horses for the coach into a roughly-constructed corral, or enclosure, about seventy or eighty feet square, made of brushwood, cut from the country around, and piled some six feet in height. Here again is a lively scene in changing mules for half-wild horses, the passengers the while stretching their legs and smoking. The cigarette forms a wonderful solace to the South American traveller; it whiles away delays, makes a bond of sympathy between people from widely-separated parts of country that may be fellow-passengers, and brings out many a tale of travel and adventure.

The first stopping-place at night is at a curiously wild spot on the borders of the Indian country, a house of one storey, with thick mud or adobe walls, forming one end and part of one side of an enclosure, about a hundred feet by fifty; at the other end of which is a pair of large wooden doors; these are closed at night, and form a protection for coach and passengers.

After an evening meal of soup, followed by an 'assado,' or joint

of meat, cooked over hot embers, a dish often very savoury, with eggs, wine of Mendoza, bread and coffee, all join around the fire for a chat and smoke before going to sleep, which is truly enjoyed on camp beds under a verandah. A large fire burns all night in the middle of the enclosure, exaggerating, by its light, the forms of the gauchos in their coloured ponchos before it, or of the coach and sleepers around; making a strangely picturesque scene.

At daybreak the bugle of the conductor stirs the travellers out of a heavy sleep, to wonder where they are; and after much merriment, and some grumbling at the early start, all rise for coffee, and to make arrangements for breakfast on the road, which quickly passes the time. Away again for the open country, just as the sun is rising, and throwing his first straight rays and long shadows across the wild scene. The road now goes across a country that bears many traces of water action; now over pebbles, then over sand, until some high ground is reached, where the Sierra de San Luis first comes into view. Here the wild verbena is found in its home, and truly beautiful the patches of colour look after the dulness of the scenery left behind. The variety of the shrubs, and their richness of growth, make a fitting foreground to the scene beyond; hills in long undulations stretching as far as the eye can follow towards the west, with the Sierra de San Luis a little to the north and on the right.

This Sierra is a continuation of the line formed by the Sierras de Ullapez and de Los Llanos, stretching away north into the province of La Rioja, made up of schistose metamorphic rocks, granite, and sandstone, carrying large veins of quartz, and rich in metals—copper, lead, gold, silver, and nickel. The little town of San Luis, 3700 inhabitants, stands 2500 feet above the sea, and is at the foot of a picturesque peak rising 2000 feet above it, from the top of which, in clear weather, Aconcagua, 23,000 feet in height, can be seen nearly 200 miles to the west. The people of this province have suffered much from Indian raids and civil war; but among them may be seen many good faces, and they exhibit a dignity of manner that makes a pleasing contrast when compared with the inhabitants of some other parts of the country. Grapes are abundant, and oranges, peaches, and figs, plentiful in season. The climate of the Sierra is said to be good for consumptive patients; that of La Carolina, a town further north, and at an elevation of 4900 feet above the sea, being peculiarly suitable; the temperature here is from 60° to 70° Fahrenheit; the air is dry and bracing, the scenery fine, and good food is procured from the valleys below.

On the road from the Sierra de San Luis to the steppes of

the Andes, but little can be seen of the underlying strata, from their being covered with sand, gravel, and alluvium. On the higher ground, much gneiss and mica schist is found, especially in the Sierra de Las Palomas. The banks of the Rio Desaguadero form an interesting feature; they are composed of micaceous and quartzose sandstones, in some parts with the characteristics of mica schist, and in others crumbling away, probably from weathering, and the action of salts brought down by the waters of the river in its course from the snows of the Andes, across the Lagunas; the banks are from forty to fifty feet in height, and are composed of the micaceous and quartzose matter, which gives to the water its greyish buff colour. From this point to Mendoza, the road passes large farms, or *estancias*, mainly devoted to fattening cattle intended to be sent to Chili. At Rodeo, near to Mendoza, a lively scene may be sometimes witnessed during the shoeing of cattle previous to their journey over the Andes. A shed, set on strong posts against an adobe wall, divides two large yards; looking over the wall near the centre, a good view of the proceedings may be obtained. In a yard about one hundred feet long by fifty wide, with no fittings other than the four walls, a tall brawny gaucho in his many-coloured costume, stands close to a door swinging a lasso over his head; his action is slow and deliberate. Opposite this door, about twenty half-wild cattle are running backwards and forwards the long way of the yard, when suddenly the lasso falls gracefully over the horns of a fine animal; it stops at once; a wild jump and a bellow are the prelude to a rush at the man in front of the doorway, who stands stock still until the animal is within a few yards of him, then with the agility of a kitten he jumps aside. The animal is unable to follow, as whilst he has been coming towards the gaucho, others have quickly shortened up the lasso around a post, which now brings up the furious beast within a few feet of the entrance of the door. Here, catching sight of the men in the shoeing shed through the doorway, he makes a second rush, and is again brought up, powerless, with his head close to a post. His legs are now fastened together by soft hide straps, and with a dexterous throw he is turned over on a bed of straw, and his legs fastened to four upright posts, his head being carefully kept close to a central post whilst he is being quickly shod. We observe that the door by which the animal entered has been closed, and that on the opposite side of the yard, at the foot of the slope, a pond of water, eight or ten feet deep, and twenty feet wide, runs the whole length of a piece of wall, six feet high, and close to the nearest edge. The man who lassoed the animal now stands leaning against the wall; the shoeing being finished all get out of the way, excepting this man near

the water. The lasso is shaken from the animal's horns by a man perched over him in the roof of the shed, which sets him free, more furious than before. Looking round, he sees his old enemy, and at once rushes at him, head down. The gaucho quietly waits until the animal is close to himself and to the water, and then steps behind the wall. The impetus acquired by running down the slope prevents the ox from stopping, and he therefore goes head-foremost into the water, to splash out on the other side, with blood and temper fitly cooled before going to pasture. The animal is prevented from coming out on the same side as the man by a perpendicular bank, while the opposite one is a slope.

On approaching Mendoza distant views of the Andes are seen, and the road is for miles shaded by poplars and willows, growing close to the banks of streams employed for irrigating the farms and pastures.

The waters from the melted snows of the Andes in the Rio de Mendoza, and one of its branches, the Tutumaya, are led considerable distances for irrigation, through channels mostly by the sides of the road, and from these are tapped into the pastures, vineyards, and fields of lucerne, the last of which are protected by adobe walls of four or five feet in height.

The town of Mendoza is beautifully situated, and most picturesque; the views of the Andes to be obtained from several parts of it through streets with trees on either side being very fine, especially when the atmosphere is somewhat clouded, while the ruins of the old town on the same site, 12,000 out of 15,000 inhabitants of whom perished by the earthquake of 1861, renders more impressive the character of the distant peaks.

The waters of the baths gathered from hot springs in this neighbourhood are strongly impregnated with sulphuretted hydrogen and carbonic acid, so strongly as to make their presence recognizable at a distance of a hundred yards.

The generally flat piece of country between Mendoza and San Juan, a distance of eighty-five miles, is interesting in its wildness. The ground shows evidence of the action of water resulting from the storms that sweep over this district, bringing down detritus from the limestone and sandstone hills to the west.

The fine sands deposited in these stream-beds harden and crack into pieces that remain firmly attached to the bottom. This may account for the filling up and diversion of the streams, which has occurred so often, as to have cut up the country in all directions. In passing almost due north from Mendoza to San Juan, the rainfall diminishes, and becomes more uncertain. This may be accounted for, to some extent, by the bend westward taken by the main chain of the Andes. The great Plateau, the western slope, and the Primera

Cadena, or first chain, with the valleys formed by the Cordon de Tontal, Sierra de Zonda, and other smaller parallel ranges, receive more rain or snow than the lower country to the east. This is accounted for by the partiality of summer thunder-storms for the higher ridges and more contracted valleys. One of the most noticeable features of the high ground of the province of San Juan is the clearness of the geological sections, suggesting sudden and severe storms, carrying detritus to great distances; this is further confirmed by the hardy character of the vegetation.

The province of San Juan is situated almost wholly within the outlying spurs of the Andes, and is geologically and socially interesting. The province contains about 60,000 people, a hardy mountain race, who live mainly by farming, wine-growing, and mining. The town of San Juan, the capital, is built on the site of the old river of that name, and has been often flooded by its waters during the melting of the snows. The houses are built of adobe; but some of the public buildings, commercial houses, and private dwellings, are of burnt red brick. Two distinct methods of making these adobe houses are employed. In the first gravel, alluvium, and sand deposited by the river are mixed with water, hammered into a strong wooden framework, held by twisted ropes, about four to six feet long, two to three feet wide, and three feet deep. As soon as one block is finished, the framework is moved on far enough to join the block just completed, one end of the finished block forming one end of the box or frame within which the material is pressed. When one tier has been thus completed the framework is raised, the previous tier forming the bottom; a little moistening of the lower tier secures adherence of the one to the other. On these rough walls two kinds of plaster are laid, one of alluvium and dry mule dung or cut straw, the other of lime plaster or stucco, and on the plaster is laid lime-wash, sometimes coloured blue and buff, with other ornament, according to the means of the occupant. The tops of the houses have a thick layer of alluvium and mule-dung laid on them, and are slightly inclined to carry off rain. And it is surprising how well they keep out the wet. The thick walls further act as a protection against the heat of summer. In the other form of adobe wall, the mixture of gravel and alluvium is made into blocks about eighteen inches long, twelve inches wide, and from six to nine inches thick; these are built up after drying with some of the same mud as mortar. These houses, by the assistance of stucco and the washes of Italian masons, are made much more pleasing in appearance than the roughness of the principal materials would suggest. The domes and towers of churches are covered with blue and white tiles, or are elaborately plastered.

The people here have more of the energy and physique of a mountain race; the dryer climate developing more nervous energy. They have had for seven years a system of compulsory education in operation, and now spend between 11,000*l.* and 12,000*l.* a-year on primary schools; and in the capital they have a national college with eight or nine professors, in which a good course is given in Mathematics, Agriculture, Mining, Metallurgy, Chemistry, Law, and Literature. They have at present one pupil for every seven inhabitants.

The Peon, or labouring class, receive from 23*s.* to 33*s.* per month, with their food, consisting of flour, beef, raisins, and maize, or Indian corn; the quantities varying according to the liberality of the employer. The meat is usually made into a soup with maize, onions, and any other vegetables they can obtain; and the raisins are sometimes used in cakes, or eaten alone. Their main drink is the well-known Maté or Paraguay tea, the dry leaf of a species of holly, the *Ilex paraguayensis*, grown principally in the Republic of that name, and in Brazil. This leaf, generally in a powdered state, is put into a gourd 3 or 4 inches in diameter, from which the inside has been carefully removed, a small quantity of sugar added, hot water poured on the top of the two, and the infusion is then sucked up through a silver or electro-plated tube. This makes a refreshing drink, and has great staying properties without producing the ill effects often ascribed to tea. Wine, though cheap and good, is rarely to be procured by them, and the more expensive *aguardiente*, or brandy distilled from the wine of the country, still more rarely. They are a temperate and industrious class, and when living near the towns often add to their comforts by growing vegetables, cultivating a few vines, and drying the grapes on the tops of their houses. This last makes an important item in the business with other provinces, and with Chili, the raisins being remarkably good.

The wines of San Juan are much like those of Spain, and are free from the overloading of spirit so common in wines sold in this country.

In the pastures of lucerne irrigated by channels taken from the river, large quantities of cattle are fattened; these are bought by the farmers in the adjoining provinces, or raised on the spot, and during the summer and autumn months—there from November to April—are sent to Chili across the Andes, and to Bolivia, 700 or 800 miles to the north. Their fat condition and slow travelling enable these animals to stand the wear of such a journey. Men receiving from 3*l.* to 4*l.* only per month, are sent off in charge of 'tropas,' or herds, of cattle with other men under them at less wages. These sell the cattle, receive the money, and return with it to their

employers, thus showing their trustworthiness when placed in positions of responsibility.

In the future there can be no doubt the province of San Juan will be an important contributor of metals; the principal known mining centres are Tontal, Gualilan, Hilario, La Huerta, Castania, Guachi, Chila, and Salado.

Gualilan, one of these, is situate in a district full of geological interest. The road to it strikes a little to the north-west of the town of San Juan, across the river of that name. This river, made up of the Rio de los Patos and the Rio del Castania, the latter coming through a district in which gneiss, schists, granite, and sandstone abound, furnishes the greater part of the micaceous matter so abundant in the waters of the San Juan. On the northern side of the river, here running nearly due east, is a range of hills, the Sierra de Villagun, composed of limestones and sandstones, with a direction nearly due north and south, and a dip of from 40° to 45° west.

On the eastern side of this range ironstones and bituminous shales are found, and of a northern continuation we shall have occasion to speak subsequently. Imperfect specimens of *Ammonites communis* and *A. Bucklandi* have been found here, and there can be little doubt that we have here the Liassic group of the Jurassic system.

This range of hills, 2500 feet above the river, or 4500 feet above the sea, increases in height, like the limestone ranges west of it, towards the north, and reaches a height of 18,000 feet.

Going again to the north-west, the road crosses a desert about twenty-five or thirty miles to Talacastro, where it goes through a pass in the Sierra de Jachal. Here we meet with a limestone somewhat darker in colour, the formation much the same generally, but the layers of limestone thinner, and dipping west at an angle of 65° in the highest hills, about 1500 feet above the road. The contortions of the strata at the entrance of the pass are very remarkable, but further up the stratification becomes regular and distinct, until we reach some irregular shaly and sandstone beds, in the former of which are found 'gossans' and quartz containing gold and silver, to the amount of 1½ oz. of the former to 2 or 3 oz. of the latter.

From the summit of this pass in the 'Camina de Borros,' (donkey road), the direction of the range of hills in which the Gualilan mining region lies, can be traced away north into the Sierra de Jachal, which rises from a height of 7000 to 16,000 feet above the sea, in a distance of about forty miles. The view from the highest point of this road is extremely beautiful; a sandy desert with a scrub growth around it, and one or two

patches of green where springs and water drainage have made irrigation possible; on its eastern side is the range just described, and on the western side the high ground, 10,000 to 12,000 feet, of the commencement of the Cordon de Tontal. The hills of Gualilan form a loop bending out of and again joining the Cordon de Tontal with an intervening space of thirty-five or forty miles.

These limestone hills stand some 300 to 600 feet above the level of the detritus, and have an average bearing of nearly north and south; the stratification is distinct and regular, dipping towards the west at an angle of 46°; the upper edges stand out sharply against the sky. Against this limestone on the western side, and rising only some sixty or seventy feet above the camp, is clay-slate much split up and weathered. Both this last and the limestone are in many places cut through at right angles by an intrusive, highly siliceous granite; this granite is found up to a height of 300 or 400 feet in the limestone.

The beds of limestone vary in thickness from six inches to four feet, but may average about three feet; the strata are divided in most instances by very thin layers of crystalline carbonate of lime, and at times by sulphate of lime and by a fine powder of amorphous silica. Throughout this limestone are found cavities, called by the natives *cañas* (pipes), or 'pipe-veins,' which pass through and between the strata, as shown in the section, fig. 1, Plate I. The direction varies little from that of the enclosing limestone, but the dip is far from constant. In many places these 'pipes' open into large cavities, one of which was found to be fifty feet long, and fifteen feet wide at its base, rising between the layers of limestone to a height of 120 feet, with a varying width; many of the layers have several pipe-veins opening into them. The large cavities contain decomposed iron pyrites, and quartz carrying gold and silver in about the proportion of one to one and a half ounce of the former to two ounces of the latter, and on the floors or bottoms iron pyrites, zinc-blende, and occasionally galena; the pipe-veins also contain hydrated ferric oxide or gossan, but in less proportion, often as much as half the diameter, varying from two to fifteen feet, being filled with a compact iron pyrites with quartz running through it. The proportion of gold and silver in the gossan and pyrites of the pipe-veins is about the same as in the deposits in the larger cavities.

The quantity of zinc-blende increases as the deposits are explored in depth, say to fifty-four or fifty-six fathoms; galena occurs in small quantities only, and in most instances near the granite, with which it is often mixed; in such cases the granite becomes friable and is easily broken, the thin cracks in

its mass being filled by sulphate of lime, quartz, and hydrated oxide of iron.

The crystalline sulphate of lime, native sulphur, and amorphous silica, which are found in so many places, suggest that highly heated vapours have played an important part in the history of these deposits.

How the cavities were first formed is not apparent, but the suggestion of some geologists that acidulated water may have caused them, receives some colour from the mode of occurrence of granite in varying places. In fig. 2, Plate I., the section of a pipe-vein is shown with the granite forming the lower wall. The surface of the granite, from which the pyrites and oxide of iron can, in many places, be easily separated, looks rounded and slightly decomposed, but the pyrites is compact and firm.

The granite in several places cuts the limestone nearly at right angles, and where this has been the case, large deposits of pyrites and gossan occur. The active agent in the decomposition of pyrites has been moisture, and this action is still going on, as witnessed in pyrites found exposed in the workings. Some of this, where moisture percolating from above has reached it, has crumbled and become oxidized, the action spreading from small nuclei to the mass around.

The Spaniards and their descendants, in working these pipe-veins, both at Gualilan and at Guachi, twenty miles further north, in a continuation of the same formation, only removed the gossans, which would constitute from one-half to two-thirds of the mass, and left the solid pyrites for future workers, who should have the means of separating the sulphur. They extracted the gold and silver from these gossans by crushing the ore between stones, many of which are still found in the districts. These were of the form shown in fig. 3, Plate I. The ore selected by them, a quartzose gossan, was broken down to the size of Barcelona nuts, and placed with a little water on the lower block, a piece of limestone about three feet long and two feet wide, with the centre cut out, so as to leave sides to guide the upper block, or granite muller. Then two men moved the upper block slowly backwards and forwards over the lower one, until the ore became reduced to a powder as fine as flour; a small quantity of mercury was then added, and the grinding again continued until the mercury was supposed to have amalgamated with all the metals. The ground ore and mercury were then removed to shallow wooden pans, and washed by a free use of water; the mercury was again used in this way until it became saturated with amalgam, when the excess of mercury was removed by wringing in a soft piece of skin. The balls of amalgam thus obtained were covered with clay, and

placed in hot embers. This treatment drove off the mercury, and left a small, spongy ball of gold inside the clay coating. This process only extracted a portion of the free gold, as assays of their 'tailings' show 30 per cent only of the gold to have been extracted.

North-east of Gualilan, and on the eastern side of the Sierra de Villagun, at the junction of the beds of the Rio Flanche and Rio Chilca, which carry water during the rainy season only, is situated a remarkable bit of country. The same limestone as that found in the Sierra Villagun is crossed by the road, which then passes over barren hills of shale and sandstone to the edge of the Valley Fertil. In these hills, parallel to another limestone range in the distance, considerable deposits of bituminous shale are found. These shales vary in thickness, from six inches to eight feet, and are imbedded in yellowish grey shale. One deposit, eight feet thick, has been opened by natives in the hope of finding coal, and samples from the depth of 90 feet gave volatile matter and fixed carbon 9·6 per cent, ash, 86·90 per cent. Colour black, lustre bright, and on exposure to the atmosphere the lumps break up into thin, shaly leaflets; the ash of this shale contained gold and silver in all the samples examined, the highest quantity being $1\frac{1}{4}$ ounce of gold, and $1\frac{1}{2}$ ounce of silver per ton. The seams can be traced along the sides of the regularly stratified hills for a considerable distance (say two miles). Close to these seams are found deposits of ironstone, containing 45 per cent of that metal, and very nearly the same proportion of gold and silver as in the ash of the shale. This is a remarkable fact, and becomes still more so, when we find that the gossan from the decomposition of the pyrites in the limestone hills to the west contains the precious metals in almost the same proportion.

There is a deposit of coal at Hilario, in this province, a sample of which gave $11\frac{1}{2}$ per cent of ash. It is found in the sandstone and shaly beds there existing, and is of such a character as to lead to a well-founded hope of the people that they may have a coal of Oolitic age to assist in building up their mining and other industries, and in reducing the expenditure of their scanty store of timber.

EXPLANATION OF PLATE I.

FIG. 1. Diagrammatic section of rocks in the Gualilan district.
FIG. 2. Section of pipe-vein, with granite forming foot-wall.
FIG. 3. Primitive native mill for grinding gold ores.

METEORS AND METEOR SYSTEMS.

By W. F. DENNING, F.R.A.S., F.M.S.

NO department of scientific research has made more rapid progress of late than that relating to the phenomena of meteors. Twenty years ago our knowledge of these bodies was of the most crude, rudimentary character. Few observers had applied themselves diligently to the task of gathering and discussing observations; and though the planetary theory of shooting-stars offered the most reasonable explanation of their appearance, yet it was admitted that the subject was involved in considerable doubt, and presented many points of difficulty. The occasional apparition of brilliant detonating fireballs, the occurrence now and then of remarkable star-showers, the precipitation upon the Earth's surface of stony masses, were facts which could be verified from many independent sources, and they set men thinking to account for the strange and startling freaks of Nature as exhibited in these phenomena. Could such bodies as aerolitic stones fall from the Moon? Could they, after being expelled from the lunar craters, have met with the Earth in her orbit and come so far within her attractive influence as to have been peremptorily drawn to her surface? Could the smaller shooting-stars, often seen on starlight nights, be mere condensations of gaseous compounds in the extreme outer limits of the atmosphere, rendered combustible by unknown processes occurring at certain stages of their formation? * These, and

* Some of the ancient philosophers appear to have formed correct ideas of the astronomical nature of meteors. Humboldt says that Diogenes of Apollonia, who probably belonged to the period intermediate between Anaxagoras and Democritus, expressed the opinion that 'together with the visible stars there move other invisible ones, which are therefore without names. These sometimes fall upon the Earth, and are extinguished, as took place with the star of stone which fell at Ægos Potamoi.' Plutarch, in the *Life of Lysander*, also says, 'Falling stars are not emanations or rejected portions thrown off from the ethereal fire, which when they come into our atmosphere are extinguished after being kindled; they are rather celestial bodies which, having once had an impetus of revolution, fall, or are cast

other questions, came to the fore at first as likely to yield something tangible in the attempted elucidation of the subject, but the materials from which the investigation must be conducted were of the most imperfect, indefinite nature, and there seemed little prospect of a satisfactory solution of the question on such a basis. Records there were, and in abundance, of many exceptionally large meteors and of star-showers; but though the facts were well attested in themselves, the descriptions failed in the most important details. The observers had been startled by the unexpected character of the phenomena, and had given exaggerated accounts of what they had seen. The vivid brightness of a meteor (overpowering the lustre of the stars, and even vieing with the Moon in splendour), the flaming train left in its wake (curling itself up into grotesque shapes as it drifted and died away), the form of the nucleus with its jets and sparks, and the final explosion with the accompanying reverberations, were all portrayed by the enthusiastic observer; but in rare cases only were the really valuable points preserved for investigation. The *direction* of the meteors' flight amongst the stars, and the *duration* of its visibility, were facts of more significance than the mere appearance of such an object, which, though imposing in an extreme degree, was yet useless for calculation of the orbit. That these points were seldom given their due weight by some observers is to be regretted, though it is a matter for congratulation that at the present day men are fully alive to the importance attached to such records.

Heis at Aachen (and later at Münster), in Germany, was the first to devote himself with proper method to the habitual observation of shooting-stars. He was attracted to the subject at a time when our knowledge was meagre in the extreme, entering a path untrodden by any previous observers, and setting an example to those who followed him in the same line of research. His observations soon indicated the existence of a large number of meteor-showers, and showed that the periodical displays of August and November, though unique and specially interesting as furnishing many falling stars at their several epochs, were not by any means the only such systems requiring investigation. He was aided by the contemporary labours of Schmidt at Bonn and Athens, and other assiduous observers, who now began systematically to apply themselves to the work, cataloguing the apparent paths of the meteors visible during their watches, with the ultimate view of comparing them together and deducing the radiant points of the chief showers. Thus the work progressed until Schiaparelli of Milan enunciated

down, to the Earth, and are precipitated, not only on inhabited countries, but also and in greater numbers, beyond these, into the great sea, so that they remain concealed.'

his theory of identity in cometic and meteoric orbits. He was led to this remarkable discovery by investigations of the August meteors in 1866; and it at once raised this branch of astronomy to a much higher position than it had formerly occupied, for it gave the phenomena of shooting-stars a significance far beyond what had been accorded to them. They were shown to be purely astronomical in nature and origin, revolving in elliptical orbits about the Sun, and existing in infinite numbers in interstellar space. The constituent atoms of a shower became visible when upon the intersection of the Earth and meteor orbit they entered the atmosphere with such velocity as to produce instantaneous ignition and combustion, resulting in the immediate disruption and collapse of each particle so entering into collision. Only in rare cases and in the instance of the more massive fragments would one be found of sufficient solidity to penetrate completely through the atmosphere and fall upon the Earth. By far the greater number of such bodies being of miniature size were consumed in the upper rarefied limits of the atmosphere, and obviously that would be an exceptional incident in which a meteor overcame the resistance of the dense air strata lying closely over the surface of the Earth.

The intimate relation of comets and meteors being clearly demonstrated by their accordant orbits and by the occurrence of star-showers at the very times when the Earth passed through cometary tracks, it was sought to establish the theory on the basis of a large number of such agreements. But apart from the meteoric displays of April 20, August 10, November 13 and 27, there are no instances, which can be safely and certainly accepted, of exact coincidence in the calculated orbits of meteors and comets; and this is a little surprising when we admit into the comparison the large number of results which have accumulated. In many cases the positions of the radiant points agree closely, but the shower of meteors appears to be diffused over a long period, with no evidence of condensation on the special night when the Earth approaches the node. Take the case of Comet I. 1850 ☋, radiant point at 313°·5 + 60°·5 on June 23+. Now there has never been observed a well-defined outburst of meteors on or about that date, though from the point mentioned a lingering shower (or series of showers) is sustained during the long interval from April to November with an average radiant at 315°·5 + 60°·0, derived from twenty-three independent observations. Another instance of like nature is afforded by Comet II. 1850 ☋, radiant at 2° + 54° Oct. 19+, which falls very near a series of showers near *a Cassiopeiæ* at 7°·2 + 52°·1 July—Nov. (twenty-two observations). Now the inference of physical connexion between these pairs of radiant points is rendered untenable by the remarkable persistency of the

meteoric display; for a true cometary shower can only endure a few nights. The agreements, therefore, are conjectured to result from pure accident, for obviously there must arise many similar positions amongst the large number of radiant points at present comparable. This is rendered still more probable by the fact, that even in some cases where the accordance is good, both in position and epoch of the radiant centre, the comet's paths were very distant from the Earth, never approaching it within many millions of miles, so that it is difficult to understand how the particles are encountered at such remote distances from the nucleus. A shower of meteors from *Corona* in April was found to coincide with the radiant point of the Great Comet of 1847; but the accordance is otherwise defective and vitiated by the fact that the comet's nucleus 'in its node and perihelion almost grazed the body of the Sun: and only the lengthy tail which it swept or wheeled round with it can be supposed to have reached the orbit of the Earth.' * Numerous other instances might be cited in which the orbits of comets falling far within the Earth's path cannot possibly occasion a meteor-shower, unless, as Weiss and Schiaparelli have considered likely, 'some portions of the cometary substance, repelled from their proper orbits by the Sun in the form of the tail and other luminous appendages emitted by the comets near their perihelion passages, may have extended to such a distance in their orbit planes as to intersect the orbit of the Earth.' † We are ignorant at present of all the conditions under which meteor-showers are produced, and of the many varieties existing among them; but it appears probable that the meteoric particles may be sometimes distributed over a considerable width of orbit by the trains and other luminous projections from comets during their circumsolar passages.

The apparent long duration of meteor radiant points has struck nearly every observer who has entered fully into this department. Mr. Greg has pointed out, that according to his own experience, the average duration is far beyond the limits considered probable.‡ He states that in his own catalogue of meteor-showers deduced from 2000 shooting-stars seen in England during the years 1849–67 the average duration for forty radiant points (occurring at all periods of the year) is thirty-three days; and there are twelve showers reaching fifty-four days, some of which, however, he regards as not really one shower. He found that in not a few cases the period would seem to extend for two or three months without any special intermission. Dr. Schmidt's results confirm Mr. Greg's, for of the forty-five showers included in his catalogue the mean

* *B. A. Report on Luminous Meteors*, 1877, p. 165. † Id. 1873, p. 402.
‡ *Monthly Notices R.A.S.*, vol. xxxviii., p. 351.

duration of visibility is thirty days or more, and many instances of long duration are similarly evident amongst the extensive observations of the late Professor Heis. Schiaparelli from the reduction of the meteors seen by Zezioli at Bergamo in Italy found a series of radiants in the month of January grouped together near η—ζ *Ursæ Majoris*, and α *Coronæ*—α *Boötis*, and in April near π *Herculis*, and he regarded them as distinct or separate families of radiant points, which, though separated from each other in position or by nights on which no intermediate meteors were observed, nevertheless possessed in common some features of very close resemblance. 'Should the effect of planetary perturbations which retarded the return of Halley's Comet in the year 1759, nearly one month from the time of its perihelion passage, as calculated by D'Alembert and Clairaut, also explain the wide differences between the separate coils of spiral meteoric streams apparently encountered by the Earth in the meteor-systems of which the above groups or families of radiant points appear to present unmistakable examples, a new field of investigation in meteoric astronomy is beginning to unfold itself in these interesting discoveries.' *

In the spring of 1876, the writer commenced a series of observations of shooting stars, and they have been continued to the present time. The aggregate number of meteors observed is 5706, in about 520 hours of watching. The path-directions were always registered with special care, the chief aim being to derive the radiant points accurately. About half of the total number of meteors were recorded before midnight, and the remaining half in the morning hours. To supplement and confirm my personal observations, I undertook the reduction of a large number of the meteor-paths registered in the foreign catalogues of Heis, Weiss, Zezioli, Konkoly, and of the Italian Meteoric Association, 1872, and the number thus projected on star-charts amounts to more than 13,000. A considerable list of radiant points has been founded on this investigation, and the results, when compared with other observations, present many satisfactory accordances. The long duration of certain showers appears to be incontestably proved, for the same radiant points became manifested again and again. They cannot be distinct streams, supplementing or succeeding each other at short intervals from the same general directions, or they would exhibit considerable differences in position; whereas, in the cases referred to, the place of departure adheres tenaciously to the same exact point of the heavens, so far as it is possible for careful and prolonged observations to discriminate. The following are some of the most prominent examples of these long-enduring streams, the average centres of which are derived from a variety

* *B. A. Report*, 1871, p. 48.

of independent determinations by myself and many other observers confirming each other very exactly:—

No.	Apparent duration of the Radiant Point.	Position of Radiant Point. R. A.	Decl.	No. of Radiants averaged.	Approximate Star.
I.	July—Nov.	7°·2	+ 52°·1	22	α Cassiopeiæ.
II.	July 21—Nov. 13	7·7	+ 35·9	14	μ Andromedæ.
III.	July—Mar.	30·2	+ 36·0	20	β Trianguli.
IV.	Aug. 3—Nov. 13	43·9	+ 25·1	13	ε Arietis.
V.	July 6—Dec.	46·2	+ 45·9	22	α Persei.
VI.	Aug. 6—Nov. 12	61·2	+ 36·4	16	ε Persei.
VII.	July 25—Dec. 6	62·0	+ 48·0	12	μ Persei.
VIII.	July 25—Dec. 8	69·5	+ 65·2	10	c Camelopardi.
IX.	Aug. 6—Dec. 27	76·1	+ 32·2	19	β Tauri.
X.	Aug. 29—Dec.	80·4	+ 22·7	14	ζ Tauri.
XI.	Sept. 15—Jan.	106·1	+ 11·9	13	β Canis Min.
XII.	Sept.—Dec.	108·5	+ 24·2	15	δ Geminorum.
XIII.	Sept.—Feb.	132·3	+ 46·9	21	χ Ursæ Maj.
XIV.	Oct. 11—Feb. 16	132·5	+ 20·8	11	δ Cancri.
XV.	Nov. 1—April 12	181·1	+ 34·7	15	γ Comæ.
XVI.	April 21—Dec.	282·7	+ 57·0	21	ο Draconis.
XVII.	July 26—Oct. 31	290·6	+ 69·7	14	δ Draconis.
XVIII.	April 19—Nov. 15	315·5	+ 60·0	23	α Cephei.
XIX.	June—Oct.	332·8	+ 48·6	17	Lacerta.
XX.	June—Dec.	335·7	+ 62·7	17	ξ Cephei.

These average positions each represent the focus of a series of well-defined meteor-showers, clustering near together at successive epochs. The following diagram shows, in two cases, (Nos. III. and V.), the individual radiants from which they are derived, which may be taken as fair samples of the whole:—

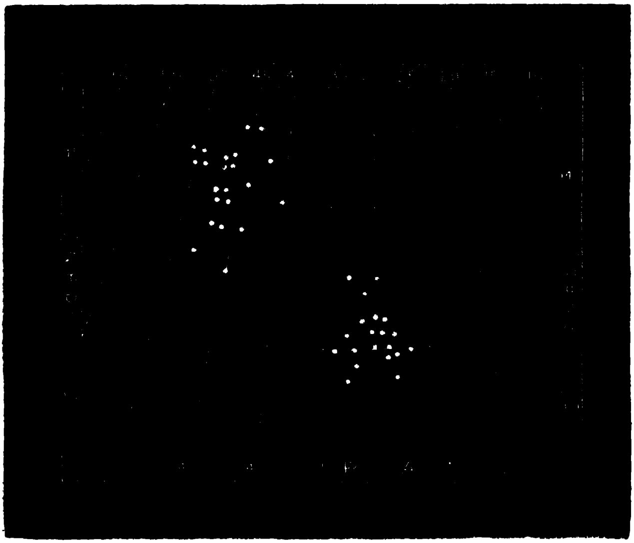

FIG. 1. Radiant points of meteor-showers near β Trianguli and α Persei (30°·2 +36°·0 and 46°·2+45°·9).

The shower, No. III., near β *Trianguli*, is $1\frac{1}{2}°$ N. of that star, and nearly all the meteors diverging from it have been short, appearing amongst the well-known stars of *Perseus*, *Auriga*, and *Andromeda*. At first they are very swift, streak-leaving meteors, but at each later manifestation the velocity decreases, and they are rarely accompanied by streaks. It is a remarkable fact, that though I have seen 4400 meteors (exclusive of the August *Perseids*) during the last six months of the years 1876–79, nearly all of them amongst the constellations included in the zone from *Pegasus—Lacerta* to *Leo—Ursa*, yet I have never found a meteor-shower precisely at β *Trianguli*, or slightly below that star, or to the east or west of it. Over and over again the radiant point reappears at the same place, so far as the necessarily approximate nature of the observations will allow one to determine. From *Auriga*, too, the meteors constantly fall from a centre about 2° E of a line joining β *Tauri* and ι *Auriga*,* and below an elongated group of small small stars there. (See No. IX.) In *Perseus* there are also several well-determined points of departure extending over a few months; and it now remains for observers to further investigate the alleged peculiarities. The importance of the subject cannot be over-estimated. It is impossible that meteor-showers can last longer than a few nights (except under special conditions), on the assumption that they form parabolic orbits, and that the true cometary-meteor-showers of August 10, November 13 and 27, &c., are to be accepted as typical streams belonging to the solar system. Unless the meteor-rings are of great width, or unless their orbital directions are nearly parallel with the Earth's motion, it is certain that the showers must be very short-lived, because the Earth's orbital velocity of about $18\frac{1}{2}$ miles per second carries her on about $1\frac{1}{2}$ million of miles in a day, and she must quickly traverse the stream intercepting her path, unless, indeed, it is diffused over a vast area of space, in which case the showers might possibly be sustained several months, but not without a change in the position of the radiant point. Evidently the identity of a comet with a star-shower is only safely to be inferred when the position and epochs of the two radiants are in exact conformity. Amongst the very large number of such systems existing in space, there must be many varieties of orbit and origin, and the complicated effect of planetary perturbations upon such slender streams is perhaps not yet sufficiently known or appreciated in its full significance. Towards the ultimate explanation of such anomalies as observations have induced, we require much further assiduous labour in the same field.

Those who have worked in this department of astronomy will

* There is an equally persistent centre 25° N. of this at 77° + 57°.

already understand how extremely difficult it is to obtain accurate and certain results. The infinite number of meteor-streams, and their great feebleness in general, and the difficulty of avoiding error in recording the path-directions of shooting-stars, and ascribing them their true radiants, all render the subject very complicated, and are a source of constant embarrassment to the observer. Moreover, the frequent impediments offered by cloudy weather and moonlight prevent anything like extensive observations being obtained during a single year, though a person constantly on the alert for all available opportunities ought certainly to register between 4000 and 5000 meteors in the course of twelve months, presuming that he worked all night long, and that the period of his observations was ordinarily favourable in point of weather. But extensive work of this kind is almost useless, unless it is thoroughly reliable. Even slight errors in mapping the meteor-paths bring about great discordances, and introduce confusion into a subject the nature of which is confused. The radiant points, deduced from hastily-gathered materials, appear to be extended over considerable areas of the sky, and cannot be determined with the necessary precision. It is a good plan, and one likely to conduce to correct results in observing meteor-flights, to project a perfectly straight rod or wand (held in the hand for the purpose) upon the apparent course of a meteor directly it is seen, and then, noting the path exactly, it may be transferred to the star-chart. By this method the true direction is very closely represented, and it is necessary to adopt some such means of aiding the eye, which often fails to retain more than an approximate idea of the apparent course amongst the stars. The proposed plan so far facilitates precision, that in the case of swift, streak-leaving meteors the paths may often be registered with instrumental exactness, for in the moment that the streak lingers on the course, the wand is projected upon it, and the line of flight carefully noted and accurately reproduced on the star-map.

It is only by the exercise of the utmost attainable accuracy that the numerous distinct showers visible every night of the year may be disconnected from each other, and their radiation-centres ascertained. Observations continued all night, or during at least four or five hours, are very valuable in this connexion; for the majority of the showers are of such extreme tenuity, that during a short watch of, say two hours, no sign of their operation may be evident. It is to long-continued and accurate observations that we must look for important results, and for the final solution of the difficulty presented by the apparent long endurance of meteor-showers. Records of stationary meteors (*i. e.*, meteors whose direction of motion

coincides with the line of sight) are also of great utility, as they obviously appear in the immediate region of the radiant point. Large meteors, such as bolides and fireballs, when observed at several different stations, offer another means of deriving true radiant points, though it is not often that the descriptions of those who witness such brilliant objects are found to agree and to admit of satisfactory comparison. But when, as occasionally happens, the phenomenon is independently recorded by several practised observers, the orbital elements may sometimes be derived with similar accuracy to those of a well-observed comet. The radiant points of large meteors thus derived are strictly comparable with the ordinary feeble showers of small shooting-stars, because there is no doubt they proceed from identical systems. The *Perseids* of August, and the *Leonids* of November, supply meteors of all magnitudes, from the faintest class just within the limits of vision up to the largest type of fireballs. Numerous other systems, amongst which may be mentioned the November *Taurids*, also display the same variety in the brilliancy of their members. The great detonating fireball of November 23, 1877, whose radiant point was very exactly defined (by five good descriptions of the line of flight) in *Taurus* at 62° + 21° belonged to the system of *Taurids* noted by Mr. Greg as a conspicuous shower in November, and well seen by the writer on November 20, 1876, at the point [illegible]° + 22½°. The large detonating meteor seen in the early evening of November 21, 1865, with a radiant point at [illegible]° + 13° appears also to have belonged to the same stream. The great fireball of May 12, 1878, nearly coincided in its radiant point (211° − 7°) with a well-determined radiant at 207° − 8° in April—May; and on August 11, 1876, a large meteor was seen at many places, the recorded paths indicating a good radiant point at 60° + 51°, corresponding with a shower traced by the writer at 60° + 50° on August 12, 1877. Many other corroborative radiants of large fireballs, with ordinary star-showers, might be adduced to confirm the idea that they belong to the same individual systems, though in some cases, it must be admitted, the agreements may be accidental. Prof. von Niessl regards detonating and aerolitic meteors as a distinct class of cosmical bodies, which differ from comets and periodical meteor-showers in the original velocities with which they enter the sphere of the Sun's attraction. He has assigned hyperbolic elements to the orbits of several fireballs, and expresses his conviction that they have native velocities in space bringing them from remote star-spheres into the neighbourhood of the solar system.*

It is fair to conjecture that, amongst the vast assemblage of

* *B. A. Report*, 1877, p. 146.

meteor-streams annually intersected by the Earth, there are many varieties of orbit, from the hyperbola to the nearly circular orbit, calculated for the fireball of Nov. 27, 1877, by Major Tupman. The observed time of flight is the important element in the determination of meteoric orbits; and it is unfortunate that accuracy is very seldom attained in this respect, and that sufficient attention is rarely given by observers to the estimation of a meteor's visible duration. As to the number of meteor-showers at present ascertained, it is impossible to specify the exact figures. Several hundreds of well-defined systems are now included in our catalogues, and this department is enriched every year with new additions to its already extensive records. Previously unsuspected showers are constantly being brought to light by assiduous observations; new meteor-epochs of decided intensity are becoming well established; and many of the old showers receive frequent and ample confirmation during their annual recurrences. But though much has been achieved, more remains to be done in the future, for we are only just beginning to unravel the complicated details of the subject. Meteoric astronomy is but in its infancy. While other departments of Science can number their centuries of research, this branch can only go back a few years to the time when Heis first gathered the materials for its primitive elucidation, and the subject is so wide and difficult, requiring ages of diligent observation to explain the intricate and anomalous points it opens up for investigation, that it will be very long before we obtain a full knowledge of the principal meteor-swarms in their varied aspects, for they are as numerous as the stars in the sky; and the extremely feeble nature of their apparition will for many years enable most of them to elude detection. The intermittent character of the true periodical showers will also allow them in some cases to escape the most vigilant eye, for becoming visible at very short intervals only once or twice in a long period of years they are very likely to be overlooked, though their special intensity will generally afford the means of their discovery.

Meteors proceeding from the same stream present great similarity in their visible appearances, and if the observer is careful to note this in the case of each meteor seen, he will often be able to attribute the true radiant point though the path may be directed from the positions of several contemporary showers. Thus the *Perseids* of August 10 and the *Leonids* of November 13 are of great swiftness, frequently bright, and with the almost invariable accompaniment of streaks. The *Orionids* of October 20 are of the same character. The *Geminids* of December 10–12 are swift, short meteors without streaks. The *Taurids* of November are slow, sometimes trained, and

often brilliant; and in the early evenings of that month, when the radiant is at a low altitude, they are extremely conspicuous objects with inordinately long courses. The *Andromedes* of November 27 are very slow meteors, and not so brilliant generally as the *Perseids* and *Leonids*. In fact, the two great showers of November are utterly dissimilar in many points, though each can boast a parent comet, and each can give a periodical display of exceptional grandeur.

It must be remembered that the appearance of meteors is a good deal affected by the position of the radiant points, astronomically and sensibly. Showers directed from the neighbourhood of the Earth's apex are characterized by great swiftness and enduring streaks, but the non-apical meteors are of less speed, according as they recede from the apex, and phosphorescent streaks are rarely seen upon their courses. The position of a shower sensibly, and often materially, modifies the distinctive features of its members. When the radiant is on the horizon, the apparent paths are of extreme length; but a radiant at great altitude, near the zenith, will furnish meteors with short, diving courses. These changes are due to the effects of perspective, and it is evident therefore that observers should carefully regard these facts in determining the real diverging centres of showers, for during the same night the meteors of one family undergo striking variations as the sensible position of the radiant alters its situation with respect to the observer's horizon. This will become apparent at once if the *Leonids* are watched from their rising on the night of November 13. At first the conforming meteors are of considerable length, and their flights are somewhat gradual across the sky; but just before daylight, when the stars of *Leo* are approaching the meridian, their short, darting courses are in singular contrast to the earlier apparitions. When the radiant is very low, the visible paths are occasionally extended over a vast space of the heavens. On November 12, 1879, at 10.17 p.m., the writer observed a *Leonid* (though the constellation of *Leo* was below the horizon) with a path of 98°, and this, with a single exception, was the longest track ever recorded by him. On December 13, 1876, a meteor was seen at $7^h\ 28^m$ p.m. with a path of 122° directed from a shower in *Leo Minor* just rising. The average lengths of meteor-paths would appear to be about 11°. The mean assigned by Coulvier-Gravier is 13°·9. Schiaparelli deduced from Tupman's observations an average of 11°·0. The writer at Bristol finds 11°·23 from his own observations,* and Sawyer at Boston, Mass., gives 9°·8.

But the mean apparent *lengths* of the paths are of minor importance to the *directions* of the paths, and it is to be hoped

* Deduced from 3203 meteor tracks registered in 1876–78. Of 661 tracks since recorded n 1879, the mean length is 11°·34.

that at some future time when such records have multiplied an attempt will be made at a complete reduction, with the idea of distinguishing the showers common to each night of the year. We have already gathered a sufficiency of materials for certain periods, viz., April 18–20, July 25–31, August 5–12, October 18–22, November 10–13, &c., when observers have been diligently watching the occurrence of the special showers of those epochs. In August particularly a large number of records have accumulated, and chiefly on the few nights when the *Perseids* offer an abundant display; in fact, about two-fifths of the aggregate mass of shooting-star observations are for this period alone, and it is now important that the work should be more equally distributed over the year. Many nights have been almost wholly neglected, and many suspected showers have received meagre support. The more slenderly ascertained epochs now demand investigation, equally with those already well-authenticated by numerous observations.

Certain nights of the year show a marked predominance of fireballs and meteors of the brilliant class. The writer recently tabulated about 3600 such observations with the view of finding the special dates of frequency, which were indicated as follows:*

January **2**, 21, 31.
February 3, **7**, 10.
March 1, 2, 4.
April **11–12**, **19–20**.
May 2, 4, 15, 31.
June **6**-7, 12, 29–30.
July 11, 20–21, **25–30**.
August 3, 5, **7–13**, 15, 19–22.
September **1–2**, **6–7**, 11–13, 25.
October 13, 15, 17–18, 22, 24, 29.
Nov. **1–2**, 4, **6**-9, **11–15**, **19**, **27**.
December **8**-9, **11–12**, **21**.

The most remarkable of these dates are distinguished by larger type; they agree in many instances with the epochs of prominent meteor-showers.

From the extensive lists of radiant points published during the last few years, it is difficult to select those positions offering the most abundant displays; but it is believed that the following table (necessarily very incomplete) includes the chief showers observed or reduced by the writer during the last few years.

Epoch.	R. A.	Decl.	Epoch.	R. A.	Decl.
Jan. 1	230°	+ 51°*	Mar. 14	263°	+ 48°
Jan. 9 and Dec. 10–13	221	+ 41	April 9–12	212	+ 65
			April 9–12	184	+ 59
Jan. 14	130	+ 44	April 19–20	274	+ 37*
Jan. 17	295	+ 53	April 19–23	288	+ 22
Feb. 15–20	236	+ 11	April 19–23	295	+ 42
Feb. 20	181	+ 34	April 19–23	280	+ 58
Feb. 20	263	+ 36	April-May	229	− 4
Mar.	175	+ 10	May	210	− 10
Mar. 14 and April 9–12	249	+ 51	July 11, 25–31, and Aug. 10	6	+ 37

* *The Observatory*, vol. iii. pp. 127–32.

Epoch.	R.A.	Decl.	Epoch.	R.A.	Decl.
July 27–30 & Aug.	341°	– 13°*	Oct. 8	103°	+ 12°
July 28–30	326	– 12*	Oct. 15	133	+ 20
July 30—Aug. 1	32	+ 53*	Oct. 15–16	31	+ 9
July 29 and Aug. 4 and Sept. 14–15	30	+ 36	Oct. 15–22	92	+ 16*
			Oct. 15 & 30	108	+ 23
Aug. 6–13	96	+ 71	Oct. 31—Nov. 4	43	+ 22
Aug. 6–12 and Nov.	70	+ 65	Oct. 14–20	106	+ 50
Aug. 6–12 and Sept.	61	+ 39	Oct.	316	+ 59
Aug. 6–12	61	+ 48	Oct. 17–19		
Aug. 6–12	78	+ 56	Nov. 7	102	+ 72
Aug. 10	43	+ 58*	Nov. 8, 12, & 20	62	+ 22*
Aug. 12	31	+ 18	Nov. 13	148	+ 23*
Aug. 21–23	291	+ 60	Nov. 26–29	155	+ 36
Sept. 1	306	+ 54	Nov. 26	208	+ 43
Sept. 21	31	+ 19	Nov. 27	20	+ 46*
Sept.–Oct.	78	+ 57	Dec. 6	80	+ 23*
Oct. 2	225	+ 52	Dec. 8	145	+ 7
Oct. 3–4	133	+ 79	Dec. 9–12	106	+ 32*
Oct. 8 and 15	46	+ 27	Dec. 9–12	134	+ 50
Sept. 14–25 & Oct. 8 and Nov. 7	77	+ 31	Dec. 9–12	152	+ 43

An asterisk is affixed to the major showers, though it is impossible to say in several cases, which of them are entitled to precedence.

The two oldest, and certainly the most notable, of the meteor-systems of which we are at present cognizant are those of August 10 (*Perseids*) and Nov. 13 (*Leonids*). And since the magnificent star-shower of Nov. 27, 1872, we have a third specially interesting system, remarkable not only on account of its unrivalled intensity, but also on account of its probable identity with Biela's lost comet, of which it apparently forms the *débris*. Repeated search for the comet has been fruitless since 1852, and this meteor-stream occurring at the end of November, and obviously following the same orbit, is the only indication we have of its present existence. As to the shower of *Perseids* in August they form the meteor-flight of Comet III. 1862: and the *Leonids* of November present an orbital resemblance to Comet I. 1866. The former shower recurs annually with considerable activity, supplying about sixty meteors per hour (for one observer) on the night of the maximum intensity, so that the particles must be scattered pretty evenly along the orbit. But in the case of the two November streams the conditions are different. The atoms are evidently condensed about the regions of the cometary nuclei, and it is only at certain epochs (when the comets are near their nodal passages) that a rich display can be expected. The *Leonids* were seen by Humboldt in 1799 on November 12; and in the years 1833 and 1866 they reappeared with striking magnificence, so that the periodical apparitions are due at intervals of 33¼ years, and may

be next expected in 1899. But there is no doubt that though this meteor-cloud is confined in its main richness to the close region of its cometary nucleus, yet the particles are sparsely dispersed through the whole orbit. The stream may be very attenuated in places, but careful observations sustained at the proper time will reveal a few of the swift, streak-leaving meteors incontestably belonging to this shower. There were displays in 1787, 1818, 1822, 1823, 1841, and 1846, and more recently in 1877 and 1879, though in each case the comet itself was far removed from the neighbourhood of the Earth's orbit.

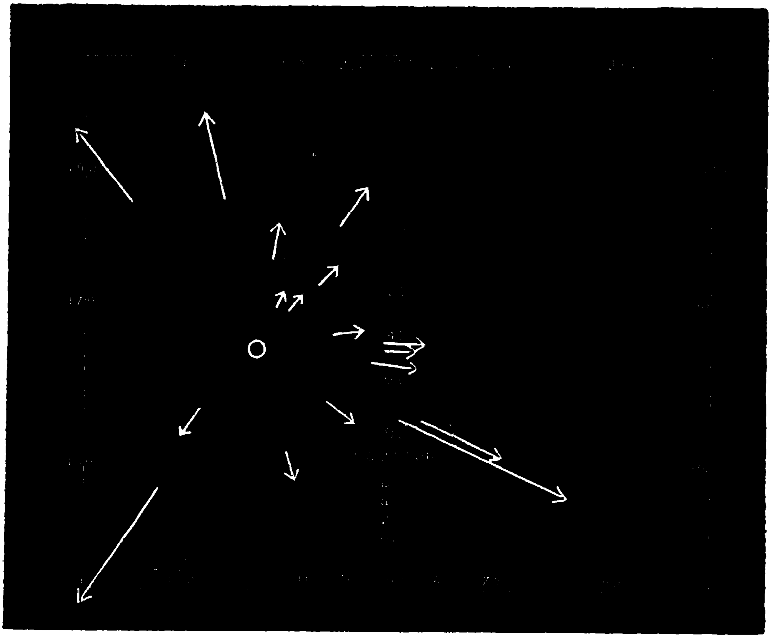

FIG. 2. Paths of 17 *Leonids* observed on the morning of Nov. 14, 1879. Radiant point = 148° + 23°.

The shower of the present year was sustained over the five nights from Nov. 11 to Nov. 15. The first meteor was seen at about 11^{h} 31^{m} on the 11th, both at Greenwich and at Writtle, leaving a fine streak amongst the stars in *Eridanus* and *Cetus;* but the paths as registered at the two stations are non-divergent, and the radiant cannot therefore be derived with certainty, though the meteor was undoubtedly a brilliant *Leonid.* On Nov. 12, Mr. H. Corder, at Writtle, in Essex, saw many meteors (in a watch of five hours), including twelve from a good centre in *Leo* at 149° + 23°, and on several other nights he traced additional paths, making 29 in all, from

the same system. They were bright meteors, eight of them appearing equal to stars of the first magnitude, and eighteen of the twenty-nine left streaks. On the night of the 13th the shower was seen by the writer at Bristol, where observations were commenced in the early evening at 5^h 30^m, and maintained until 17^h, during which period exactly 100 shooting stars were recorded. Of these, eighteen were *Leonids*; four of them were brighter than the stars, in fact the meteors generally from this shower are finer than the average. The radiant point was very distinctly shown at 148° + 23°, being extremely near Mr. Corder's position and to Professor Herschel's average centre at 149° + 23° for the great shower of Nov. 13, 1866. Every meteor left a bright streak, and the flights as registered are exhibited in the accompanying plane-perspective diagram (Fig. 2).

The maximum occurred at about 15^h, Nov. 13, when these bright *Leonids* were appearing in rapid succession; but immediately afterwards a lull ensued, and meteors came only one now and then from the sickle of *Leo*, now risen high and clear in the eastern sky. The position of the radiant as determined this year coincides exactly with previous observations, and is shown in the annexed sketch (Fig. 3):—

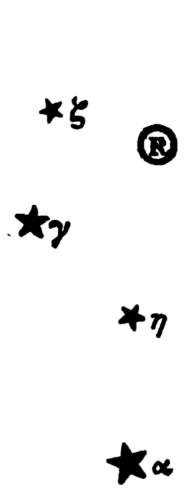

FIG. 3. Position of the radiant point (R) among the stars in the Sickle of *Leo*.

On the night of November 14, the shower was observed by Mr. Gray at Bedford, who, however, failed to see the outburst on the preceding night. He says: "Part of the sky was observed from 7^h 30^m to 8^h 30^m, and from 9^h 30^m to 18^h. Thirty-eight meteors were seen; at 14^h and 17^h they followed

in somewhat quick succession—ten were notably bright, and one equalled *Venus* in size and brilliancy. Of those whose paths were recorded most radiated from *Leo*; of the remainder many were traceable from the region around α and β *Geminorum*, but I was unable to find the exact position of the radiant point."

On the following night, Mr. Sawyer, at Cambridgeport, Mass., observed seven *Leonids* in a watch of two hours, and found the radiant point at 151° + 22°. Other reports from America confirm the reappearance of the shower on the mornings of November 13 and 14.

I have been particular to mention the facts of the recent display of *Leonids*, because it goes far to prove that they form a continuous stream, like the *Perseids*, and that the shower is visible every year with more or less intensity. At the present time the parent comet is nearing its aphelion (close to the orbit of *Uranus*), a vast distance from the Earth, and none but the most widely separated of its particles could have encountered the Earth at her recent approach to the node. It is a significant fact that now (thirteen years after the maximum in 1866) the shower has recurred on several successive nights in the most decided manner.

The remarkable display from near γ *Andromedæ* due at about the end of November, and having no sort of connexion with the *Leonids* to which we have been referring, was first witnessed on December 7, 1798, by Brandes whilst travelling near Hamburg in Germany. Four hundred meteors appeared in half of the sky during a few hours. On the same date in 1830 many shooting-stars were seen by the Abbé Raillard in France. On December 6, 1838, a meteor-shower was observed by Paul Flaugergues at Toulon, and by Herrick in the United States. At the Brussels Observatory meteors were recorded to be four times as numerous as on an ordinary night. On December 8, 1847, Heis saw a meteoric shower with a radiant at 25° + 40°; and on November 30, 1867, Zezioli noted a few meteors from a centre derived by Schiaparelli at 17° + 48°. On November 27, 1872, a great shower of meteors was seen in many parts from a radiant point at 25° + 43°. They fell so thickly during several hours that it was almost impossible to record the approximate numbers. During the next few years the shower became quiescent, but in 1877, November 25, the shower recurred again, and was slightly seen by the writer. On the 27th it continued feebly in progress, but the exhibition was a very meagre one. In 1878 a conspicuous apparition was looked for, but astronomers were disappointed. The shower failing in 1878 it was confidently asserted that in 1879 the conditions being more favourable, we should again become immersed in the dense portion of the meteor flight;

but nothing was seen on the predicted nights, indeed signal failure everywhere attended the observers. At Bristol, on the 27th, the sky was clear at intervals, and a carefully directed watch was instituted at several periods of the night, but no meteors were traced in the bright moonlight. The sky was cloudy on the 26th and 28th, when possibly there may have occurred some revival of the shower, or it may have taken place in the day-time, but it is certain that nothing of it was seen in England. Reports from foreign observatories* may perhaps be more favourable, but in the moonlit sky at the end of November observers had a poor prospect of seeing many meteors, unless indeed a very conspicuous display occurred. Thus the shower of meteors appears, like its derivative comet, to have eluded the search of astronomers; and in the event of its non-recovery in future years it will be conjectured that the stream has been deflected away from the neighbourhood of the Earth's orbit under the influence of planetary perturbations.

Apart from the *Leonids* and *Andromedes* of November, there are many other systems presented during that month; of these the *Taurids* and a bordering shower in *Musca* supply the chief examples, and they have afforded an abundant supply of meteors during the past month. Mr. Corder says: 'Several clear nights in the first half of November allowed of good watches for the *Taurids*, but the number seen was not large considering the length of the observations. There are some slow meteor-radiants near to this main stream which may possibly be connected, but of the true *Taurids* fifty-three were seen, and a total of eighty-five when the branch radiants are included. Of this number six were first mags., and they were curious, for they left very narrow red streaks, which is not usual with slow trained meteors. They did not appear like the gaseous streaks of the *Leonids*, but were extremely faint as if only from red-hot ash. The colour of the largest ones was greenish yellow; many of the small ones were reddish. Radiant point at $58° + 21°$, and others in *Musca*, the chief, at $47° + 23°$.' These observations were fully corroborated at Bristol on the nights of November 12-14, when two very active showers were seen from the points $60° + 21°$ and $46° + 21°$, and there were a large number of minor showers in play at the same epoch. Observers should watch for the annual returns of these numerous *Taurids* and for the other co-operative displays of the season. The *Leonids* and *Andromedes* must be diligently looked for every year, so that we may learn more of their history and store up the materials for future discussion and investigation.

* Mr. Sawyer writes from Cambridgeport, Mass., that he obtained frequent observations between Nov. 23–30 and Dec. 1–5, but there was no sign of the expected meteors.

THE LAW OF ASSOCIATION IN THE ANIMAL KINGDOM.*

By M. EDM. PERRIER.

ONE of the characteristics of the course of instruction at the Museum, has always been that it exercised a considerable influence upon the men who have to carry it out. Compelled by the very nature of the institution to keep himself constantly acquainted with what is known and what is sought, with the definitive acquisitions of science and the objects of her aspirations, obliged to co-ordinate the most recent discoveries with those which preceded them, to test all new theories and ideas, and to bind together the materials which incessantly accumulate about the stones forming the vast edifice of science, the professor sees the lines of that edifice become gradually modified, contributes personally to the accomplishment of their metamorphosis, and sometimes concludes his course under the domination of ideas quite different from those which inspired him at starting.

I admit without reservation that I have undergone this influence. Last year I commenced a series of investigations upon transformism. I had no prejudices with regard to this doctrine. If certain general ideas attracted me towards it, I had also present to my mind the objections repeatedly urged against it by the most illustrious of French naturalists, and among these the men for whom I have the greatest esteem and veneration. It seemed to me, however, in the course of my lectures that these objections were by no means insurmountable, that they were directed against modes of conception of the evolution of organisms which were not at all necessary, and that they left perfectly intact the actual bases of the doctrine. Ascending the series of organisms from the humblest to the

* This article, which contains a great deal of interesting information and some original and suggestive views on the relations of animal forms, is slightly abridged from M. Perrier's opening lecture at the Museum of Natural History of Paris.

most perfect, and seeking among them not differences but relationships, it seemed to me that a simple and very general law had presided over their formation; that they were derived one from the other by a constant process; and I found that I had added some additional arguments to the theory of the genealogical relationship of species.

The law which I now have to put forward may be designated the *law of association.* The process by which it has produced the greater number of organisms is the *transformation of societies into individuals.*

From the day when it was proved that every living creature was composed of microscopic corpuscles more or less resembling each other, from the day when it was seen that similar corpuscles capable of leading an independent existence constituted of themselves the simplest organisms, it has been thought that we might compare the highest animals and plants to vast associations of distinct individuals, each represented by one of these living corpuscles, one of these *cells,* to use the term adopted by anatomists. In the same animal the cells may display a great number of forms and different physiological properties. These forms and properties are not in the least degree modified by the vicinity of different cells. In the very heart of an organism each cell lives as if it were alone—that is to say, if it were possible to isolate a cell from the human body and to place it in a nutritive medium like that which normally surrounds it, that cell would continue to live, to obtain nourishment, to develope and reproduce itself: in a word, it would exercise all its physiological functions precisely as before. But more than this; in the organism itself the life of each cell is so independent of that of its neighbours that it is possible to kill all the cells of the same kind without affecting the others. Claude Bernard has demonstrated that curare poisons the elements which terminate the motor nerves, thus abolishing all movement, without in the least injuring the other parts of the nervous system, and, in particular, leaving sensibility absolutely intact. It was in consequence of his investigations upon curare that he asserted, more distinctly than had ever been done before, the principle of the *independence of the anatomical elements.*

Thus, in organisms, not only are the elementary individuals sometimes very dissimilar, but in spite of the bonds which unite them, they retain all their personality. We may therefore compare an animal or a plant to a populous city in which flourish numerous corporations, the members of which, each on his own account, practise some particular art or industry, and yet contribute to the general prosperity through the activity of the exchanges which occur throughout; in the higher organisms a

peculiar corporation, incessantly in action, is the ordinary medium of these exchanges; the blood globules are true traders, conveying with them in the liquid in which they float, the multifarious merchandize in which they deal.

All the comparisons that could be furnished by the degrees of relationship were employed to express the affinities presented by animals even before it was supposed that any real consanguinity existed between them; and just in the same way the organisms themselves have always been compared to societies, or societies to organisms, without any recognition that these comparisons were otherwise than purely ideal. On the other hand, we arrived last year at the conclusion that association had played an important if not an exclusive part in the gradual development of organisms; we find absolutely convincing proofs of this in the history of Polypes, and of the Vermes; the affinities of the latter to the Arthropods are doubted by no one; and we have shown how these same Vermes are connected with the Mollusca and the Vertebrata. The theory therefore extends to the whole animal kingdom.

But, in the first place, what are we to understand by association? In saying that animal organisms have been chiefly produced by the transformation of animal societies into individuals, what are we to understand by the term *societies?* Are we to say that every society of living creatures may be an individual in course of formation? Certainly not; but there are animal societies, in which the relations of the individuals are so close that each individual is not only in immediate contact, but also in histological continuity with its neighbours. To such a society we give the name of colony, which some naturalists have changed to *cormus*. The individuals composing these colonies, or *cormi*, are not always indissolubly united to each other. They may separate from their companions, live isolated for a longer or shorter time, and even form new colonies, thus indisputably asserting their independence. In the same zoological group we may find allied species, in some of which the individuals live always solitary, while in the others they are on the contrary always associated. This is the case in the very remarkable group of the Polypes or Acalephs.

One species of this group—the brown Hydra (*Hydra fusca*)—is common in our stagnant waters, and even in very small garden basins. It has never ceased to excite the interest of naturalists and philosophers since Trembley first made known its marvellous faculties. These Hydras usually live solitary, but frequently the larger individuals may be seen bearing smaller ones upon the wall of their body. In a Hydra kept in confinement the development of these may be followed step by step. They are

at first simple swellings, into the middle of which penetrates a prolongation of the body-cavity of the parent; this swelling enlarges, and speedily puts forth some tentacles; a mouth then opens in the middle of the circlet formed by the latter. The young Hydra, like its parent, is a simple sac, the wall of which is composed of a double series of cells, and of which the cavity, playing the part of stomach, communicates directly with the stomach of the parent, so that the contractions of the body may transmit any food captured by the one into the stomach of the other, and *vice versâ*. The parent and child live for a longer or shorter time in this close union; but when the young animal has attained a certain size, it detaches itself, adheres to some neighbouring leaf, and begins to hunt for prey on its own account. Very soon the parent and the young Hydra cannot be distinguished from each other, and both of them continually produce new Hydras throughout the summer.

In water abounding in food, and often in captivity, however, things go on rather differently. Each Hydra retains its progeny; the young ones increase in size, and produce new Hydras without separating from their parent. A true society is founded. Trembley for a long time kept a Hydra, which bore as many as twenty-two young ones of different generations. It was a living genealogical tree.

What is only occasionally produced in the common Hydra, becomes absolutely normal in another fresh-water species, *Cordylophora lacustris*, and in most of the marine Hydroids, in which the colonies are often composed of innumerable individuals. But then a new phenomenon occurs—the social life becomes complicated; an actual *division of labour* is effected between the members of the same colony. At first all were similar, all performed the same functions in the same manner; but speedily each individual becomes specialized. One devotes itself exclusively to the capture of food; another to the elaboration of the nutritive material; and a third to reproduction: so that in the end all these individuals, which originally had no need of one another, become mutually necessary; and by this means the society acquires a greater coherence, all its members being henceforward bound together as partners. Among the Hydractiniæ we may reckon no fewer than seven kinds of individuals, namely,—

1. The nutritive individuals, or Gastrozooids;
2. The prehensile individuals, or Dactylozooids, furnished with bunches of urticating capsules;
3. Dactylozooids without urticating organs;
4. Defensive individuals;
5. Reproductive individuals, destined to produce the sexual individuals;

6. Male individuals;
7. Female individuals.

These individuals are not distinguished only by the functions they fulfil; they are also distinguished by their external form, so that a peculiar kind of individual corresponds to each special function. Each of them, so to speak, acquires the form of its employment, at the same time rising, or becoming retrograde in the scale of organization, so that here, as in human societies, the *division of labour* superinduces differences of condition. The species thus become polymorphic.

Of the seven kinds of individuals composing a colony of Hydractiniæ, the nutritive individuals alone seem capable of being self-sufficient. The others are destitute of mouth and of tentacles; the sexual individuals are reduced to simple sacs; the defensive individuals seem to be only horny spines, between which the polypes can retract themselves. In presence of these facts, it would seem to be an exaggeration to attribute the quality of individuals to these different parts; we have here, it might be said, simple organs. But organs of what? They are just as independent of each other, just as independent of the nutritive individuals as the latter can be of one another. Hence they are not organs of those polypes. Are we to see in them organs of the colony? This is at once to recognize that the colony has an individual character, and consequently to assume the transformation that we seek to demonstrate. But how has a colony been able to acquire such organs? whence can they have arisen if not from a transformation of the individuals composing it?

However, we have no occasion for hypothesis in order to demonstrate that these *colonial organs* are the equivalents of true individuals. The buds which give origin to the different kinds of individuals in a colony of Hydractiniæ all originate in the same way, and they are for a long time so similar that there is nothing to enable one to distinguish them. This furnishes a first presumption in favour of their equivalence; but in the allied type *Podocoryne* we see the humble sac which represents the sexual individual replaced by a most active and elegant creature, much higher in organization than the Hydra itself, namely, by a Medusa, which detaches itself on its arrival at maturity, and swims about actively in the water, all the transparency of which it possesses.

These Medusæ constitute the most general form of the sexual individuals in the group of Hydroid Polypes, but they are themselves very multiform. From species to species their form is modified; we see them stop at every degree of development, sometimes becoming reduced to the condition of a mere bud, sometimes, although completely formed, abstaining from

casting themselves free, and terminating their existence in the colony from which they were born.

In one whole group of Polypes these Medusæ, or their equivalents, associate themselves with the reproductive individuals to form a new unit, a sort of small distinct colony, which may also be taken for a peculiar organ, the composition of which presents the most curious analogies with that of a flower. This reproductive apparatus has its separate chamber, the *gonangium*.

A step further, and we shall see these Medusæ, the individuality of which is, if possible, still more strongly marked than that of the Polypes, themselves descend to the rank of organs in more complex colonies.

All the colonies of Hydroids do not live attached to submarine objects. There are some which lead a vagabond existence. They have often, and not without reason, been taken for simple animals analogous to the Medusæ, and designated by the name of Siphonophora, which has remained attached to them. They sometimes attain a large size, and the variety and number, I may say the profusion of the parts of which they are composed, together with the brilliancy of their colours and the incomparable beauty of their forms, have always been a source of profound admiration both for the naturalist and the sailor. Now each of these parts is the equivalent of a Hydra or of a Medusa.

In *Agalma*, as in the Hydractiniæ, we find nutritive individuals furnished with a long tentacle, the mere contact of which produces an intense burning pain,—a kind of *fishing-line*, capable, in the larger species of Siphonophora, of capturing even fishes. Side by side with these nutritive individuals, we find others destitute of mouths, which are nothing but the reproductive individuals, in the vicinity of which are the sexual individuals, in form very like the Medusæ. All these individuals are fixed upon a common axis, floating and undulating in the water, sustained by a sort of aeriferous bell, which forms its superior extremity; two series of sterile Medusæ beneath this bell simulate a set of rowers, to which the entire colony abandons the duty of moving it about.

In every respect these various parts are so like the Hydræ and Medusæ, that we cannot for a moment refuse to accord to them the character of individuals; the *Agalma* and the other Siphonophora are, therefore, true societies or colonies. But then most of the individuals cannot, without risk of death, be separated from their companions; and, indeed, in certain cases, all co-ordinate their movements to enable the colony to accomplish certain actions. For example, the Portuguese men-of-war (*Physaliæ*) are often seen to change their course, and

then all the individuals of the colony assist in the operation. There is consequently a will that governs them, a will which can only draw the motives of its determination from the existence of a sort of social consciousness, raising the whole colony to the rank of a psychological unit. Composed of individuals each equivalent to one of those Hydræ or Medusæ which we have seen living freely and sufficient for themselves, every Siphonophore must nevertheless be regarded in its turn as a single animal, as a true individual of a higher order. Here the transformation of the colony into an individual is to a certain extent manifest. The Siphonophore is an animal, all the organs of which are so many distinct animals, each playing a certain part. Moreover, we see these animal-organs gradually become less and less independent. They approach others, arrange themselves regularly around a central individual, which predominates, and finally combine to form a creature, such as the *Porpita* or the *Velella*, in which no one would dream of seeing anything but an indivisible animal if the investigation of the allied types did not reveal its true nature.

It is thus, also, that every one hitherto has regarded the Sea-anemones and the Polypes of the madrepores and coral, as simple organisms, primary individuals, although, in our opinion, they owe their origin to a phenomenon exactly like that which has produced the *Porpitæ* and the *Velellæ*, and likewise result from the union of three kinds of Hydriform Polypes. The fine investigations of Moseley on the Polypes of the family Stylasteridæ furnish an unexpected proof of this. Considering only their calcareous parts, all these creatures seem to be true Madrepores, and the first doubt as to their actual nature was raised by Louis Agassiz with reference to the Millepores.

Between a Coralliarian and a Hydroid Polype the difference is considerable; one is a simple sac, bearing tentacles which vary in number with the species, sometimes with the individuals, but constant for each of them during the greater part of their existence; these are usually solid, simple appendages of the wall of the body. The other consists of a stomachal sac, open at the bottom, and around it are arranged hollow tentacles, the number of which increases with the age of the polype. These tentacles, which have their upper parts free and are united by their bases, thus forming the body-wall of the polype, open inwards, like the stomachal sac, into a great cavity, the periphery of which is divided into as many chambers as there are tentacles by the united walls of each two neighbouring tentacles. Upon the partitions of these chambers the reproductive apparatus is developed, and it thus seems to be contained within the body cavity of the polype, whilst in general it appears in the Hydroid Polypes in the form of an external bud.

The Coralliarian Polype is therefore constructed upon a very complicated type in comparison with the Hydroid Polype, and it is the latter type that we find realized very clearly in the Stylasteridæ. Their colonies present the polymorphism peculiar to the Hydroids. In them we constantly find nutritive individuals, or gastrozooids, purveying individuals, or dactylozooids, and reproductive individuals, or gonozooids. In *Spinipora*, *Sporadopora*, *Pliobothrius*, and *Errina*, these different kinds of individuals are perfectly independent of each other; a simple vascular network distributes among them the nourishment seized by the dactylozooids and elaborated by the nutritive individuals or gastrozooids.

But in the Millepores, the gastrozooid decidedly acquires the predominance. An important member of the colony, since it is this which prepares the nourishment for all, it attracts around it the dactylozooids and reproductive individuals; all range themselves in a circle round the principal individual, but without entering into any intimate union with it.

In *Astylus*, *Stylaster*, and *Cryptohelia*, this movement of concentration round the gastrozooid becomes still more strongly marked; a vacant place is formed beneath it; its tentacles, rendered useless by the vicinity of the dactylozooids, finally disappear; it is reduced to a simple digestive sac, around which the dactylozooids perform functions exactly like those of the tentacle of a Coralliarian Polype. Here again, each system has very decidedly its own individuality, but with another step, the dactylozooids, still distinct throughout their length, become united at the base and attach themselves to the gastrozooid, and the gonozooids follow them in this movement. These different parts are thenceforward too close together to need a special vascular system to place them in communication; the vessels which united them are no longer more than simple perforations of their wall, all opening into the vacant space situated beneath the gastrozooid, into which the gonozooids themselves also penetrate; but this whole, the most experienced naturalist would thenceforward be unable to distinguish from what we call a Coralliarian Polype.

The individual in the Coralliarian Polype is consequently an association of parts differing in form, each of which is equivalent to a Hydroid Polype.

A Coralliarian Polype furnished with twelve tentacles, is the sum of a considerable number of Hydroid polypes, namely, a gastrozooid, twelve dactylozooids, and an indeterminate number of gonozooids. It is formed by means of the Hydroid polypes, like the flower by the agency of the leaves of the plant which bears it, or better still, like the composite flower by means of its florets. The phenomenon which has produced this polype

is the same that has produced the *Porpita*, or the *Velella*,—the *formation of the colony*, or *association*, the *physiological division of labour*, the *manifestation of polymorphism*, and finally, the *concentration of the parts thus elaborated*. Such, from the morphological point of view, is the succession of the phenomena which mark the transformation of the Hydroid Polypes into *Velellæ* and Sea-anemones. The Hydroid Polypes are the raw materials which are brought into the factory, and then fashioned and definitely assembled to form these superior individualities.

Whilst these phenomena are occurring in the morphological sense, others are manifested in the physiological. The associated individuals have at first nothing in common, unless it be the nutrition, which all are capable of elaborating, but which passes from one to the other in such a manner that all the members of a colony may equally partake of it; this is in reality the commencement of solidarity, but each polype, nevertheless, possesses its own personality. It has its personal will, and does not communicate to its neighbours the sensations it experiences; we may wound, or even remove it altogether, without any consciousness on the part of its neighbours. But in proportion as the colony becomes more coherent, the sensations radiate further and further around the polype; very soon all the individuals are sensible of actions performed upon any one of them; a *colonial consciousness* is thus manifested over and above the individual consciousness; and, finally, a single will bends all the special wills to its bidding. At this moment a new individual is definitively constituted.

The transformations that we have been able to trace, step by step, in the class of Polypes, are not peculiar to those animals. It would be just as easy to show how simple forms have in the same way become associated in the great group of the Vermes, to lead up to complicated forms; it would be just as easy to recognize in this interesting group the laws to which the study of the Polypes has led us. It is already a long time since Professor Van Beneden asserted that each joint of a Tapeworm was the equivalent of a Trematode worm, a Fluke; and for a still longer period the segments of a worm, or of an insect, have been regarded by naturalists as perfectly equivalent units, all formed of the same parts, and each having an actual individuality. The name of *zoonites*, which has been applied to them, would even seem to indicate a tendency to regard them as actual *elementary animals*, which had formed colonies by their association. The faculty possessed, in certain worms, by each of these segments of individualizing itself, and forming a new colony, bears high testimony to the correctness of this view; polymorphism and the concentration of parts suffice to explain how a *Peripatus* or a Myriopod may have become trans-

formed into an Arachnid or an Insect, how the various Crustacea have issued from a common stem, and how, from another form of colony, the Annelidan series may have proceeded.

On the other hand, it has been repeatedly maintained that the Sea-urchins, the Starfishes, and the Ophiurans, were nothing but colonies of worms soldered together by the head; they are certainly colonies, but of a very peculiar nature.

Can we say the same of the Mollusca and Vertebrata, all the parts of which seem to us to be so intimately fused together, and which are the giants of creation? Are there simple forms the association of which could explain to us the marvellous organization of these superior types, as we have explained the formation of the Siphonophora, the Coralliaria, the Echinodermata, the Vermes, and the Arthropoda? This is what we have still to investigate, but it is important to remark, that whatever may be the result arrived at, the generality of the principle of *association* will not be at all invalidated. If, in opposition to the presentiments derived from our previous studies, these simple individualities never existed, we should have in fact to compare the Mollusca and the Vertebrata with the primordial individuals, the combinations of which have produced the other types, and which we may still recognize as the base of all the great groups of the animal kingdom. Now, how did these individuals themselves originate?

The Hydræ and other analogous organisms furnish our answer. We may cut a Hydra into as many pieces as we like, each of these pieces, far from dying, continues to develope itself, and, finally, reconstructs the perfect Hydra. What are we to conclude from this, if not that the different parts are independent of each other, as the polypes forming one of the lowest colonies are of their neighbours? Each cell of the Hydra is an actual individual, and the Hydra is a colony of these unicellular organisms, just as the Siphonophora are themselves colonies of Hydras. The aptitude for social life is communicated by heredity to these cells as to the polypes. Each cell, and each polype, when detached from a colony, bears within it, as it were, the *effigies* of the colony, and its subsequent development tends always to the reconstruction of the latter. At first, all the members of a colony are equally apt to reproduce it; thus this faculty, like the others, becomes localized and tends to become the appanage of certain individuals or of certain parts; at the same time sexual reproduction gradually increases in importance; and when the society has attained a certain degree of coherence its different parts cease to be able to live independently of each other. As in the case of old people who cannot be separated after a long existence

together without exposing them to death, the different parts of a highly individualized society die without being able to reconstruct themselves; the reproductive function is thus exclusively reduced to its sexual form.

The Sponges reveal to us their colonial nature even more distinctly than the Hydraids. In them, in fact, the *Spongiarian individual* is certainly constituted by two kinds of *cellular individuals*, the Amœba and the flagellate Infusory, the analogues of which are to be met with at liberty in the state of isolated individuals. The flagelliferous cells of Sponges present quite exceptional peculiarities; they are furnished with a nucleus, and a contractile vesicle, and their flagellum is surrounded by a membranous collar in the form of a funnel. All these characters are to be met with in the *Codosigæ*,* which are unicellular Infusoria that always live isolated, and which in consequence are to the Sponges what the Hydras are to the Siphonophora and Coralliaria. In the *Anthophysæ* these cells always live in colonies, but they are still all alike. Let polymorphism step in,—let some of the associated cells retain the flagelliferous form, whilst others become Amœbæ (a transformation which is quite possible, since it is one of the most frequent modes of reproduction of the amœboid Infusoria), and the *Anthophysa* is transformed into a sponge. Thus the process is always the same; whatever may be the materials which Nature brings together, cells or polypes, she subjects them all to the same elaboration in order to obtain from them new individuals.

The cells, when once brought together, may moreover, within the organisms containing them, undergo in a high degree those metamorphoses which are the consequence of the physiological division of labour, and form diverse organs, although these organs cannot be regarded as actual individuals. If the individuals of a colony often descend to the state of organs, we must not conclude from this that the organs of an animal are always individuals which have lost their autonomy; but the animal to which they belong, although it may never have been an assemblage of individualities intermediate between its own and that of the cells, is none the less a colony of the latter, subjected to the laws of evolution of all the others.

Thus, even if it should be proved that the Vertebrata and the Mollusca are not the result of the fusion of simpler organisms which have once been able to lead an independent life, they would be none the less colonies of cells, and the *law of association* would lose none of its generality.

It remains the fundamental law of the development of the

* See an article by Mr. W. Savile Kent, in *Popular Science Review*, N. S., Vol. II., p. 113, with illustrations.

animal kingdom, including and governing those *laws of growth*, of *organic repetition*, and of *economy*, which have long since impressed the minds of physiologists, explaining the *homologies*, hitherto so mysterious, which are observed between the different parts of the body and the different kinds of limbs of the same animal, and bringing into a single group all the forms of sexual generation, which are its most powerful means of creation. Supported upon the law of the *physiological division of labour*, the importance of which was first demonstrated by M. Milne-Edwards, and upon that of *polymorphism*, which, without it have no precise meaning, and can only have a very restricted bearing, and in its turn the consequence of the *law of division* of protoplasmic masses, it has been the great producer of organisms, and establishes, in an unexpected manner, a fresh bond between sociology and those branches of biology which relate to the constitution and functions of organisms.

We now come to the ultimate elements of living bodies, the materials which have served for the construction of the simplest of them, and we may ask ourselves what may have been their origin. Here we are in the presence of unity; there is no longer any question of association. Most living cells consist of four parts; an enveloping membrane and semi-fluid contents, in the midst of which we observe a peculiar globule, the nucleus, itself containing a nucleolus. Of these four parts, only one is truly indispensable, namely, the semi-fluid contents, which are either perfectly limpid, or finely granular, the *protoplasm*. It is in this curious substance that resides life, which has no need of any other apparatus to manifest its essential characters. Some particularly remarkable creatures, the Monera, are exclusively formed of it. These are simple, perfectly homogeneous clots of a limpid jelly, like white of egg. This jelly, nevertheless, performs movements, captures animals, digests them, incorporates them with its substance, enlarges, and divides, when it has attained a certain size, into two or more small masses, which recommence the life of their parent, and like it divide when they have reached a certain size.

This faculty of division is an important property of protoplasm, for it governs all organic evolution. A protoplasmic mass cannot exceed a determinate size. When it attains this size it divides, and as its mass is perfectly homogeneous, as it is constantly acted upon by currents which mingle the different parts of its substance, the fragments produced by this division possess all the acquired or inherited properties of the protoplasmic clot of which they formed parts. This is the whole explanation of the important phenomenon of *heredity*, by virtue of which every organism transmits to its progeny (even in the

case of sexual generation) all its specific, and even a part of its personal characters.

From this incapacity of the protoplasmic masses to exceed a certain size, it follows, necessarily, that all the creatures which exceed this size, must be formed of several distinct masses of protoplasm, and, in one word, will be colonies. Thus the generality of the law of association is shown to be a consequence of one of the fundamental properties of protoplasm.

The latter constantly breaking up into distinct masses, these masses, which there is nothing to bind together more directly, are each modified in a particular way under the influence of external agents, and thus has been introduced into nature that marvellous variety which strikes us with admiration; it is an immediate consequence, like the law of association, of the obligation imposed upon protoplasm to divide into small distinct individualities.

Now what is the nature of protoplasm? Struck with its homogeneity, and with the identity of the elements composing it with those which form albumenoid substances, it has been thought that it was a mere chemical compound, and it has even been a question whether it might not be possible to produce it artificially—whether man might not be able to rekindle the torch of Prometheus and create life at his own pleasure—a serious question, which, I believe, has only been put in consequence of a strange confusion of words. If it be true that the substances which form living matter are the same as those which enter into certain chemical compounds, we cannot conclude from this that protoplasm is one of these compounds. The characteristic of a chemical compound is fixity of composition; the composition of protoplasm, on the contrary, changes incessantly, without any of its fundamental properties being modified. New substances are constantly entering into its mass, while others issue from it; protoplasm is in a state of perpetual decomposition and recomposition; it is not its chemical composition that characterizes it, but the mode in which it is incessantly decomposing and recomposing itself; everything in it is in movement, and, properly speaking, it is this movement that characterizes life.

Life is therefore nothing but a combination of motions, or, if it be preferred, a mode of motion of which certain substances alone are capable, and which is not without some analogies with those whirling motions to which certain eminent physicists ascribe the formation and the properties of the atoms of chemistry. We might follow out this comparison between the atoms and protoplasms, and employ it to show that the latter must have been formed originally in the greatest possible number, that it

would appear that we must always be incapable of reproducing them, that they appeared with a whole host of properties which have governed all their subsequent destiny, and that they showed themselves from the first with the individuality that we see in them at the present day.

THE DINOSAURIA.*

By Professor H. G. SEELEY, F.R.S., F.G.S.

[Plate II.]

Exactly fifty years ago Hermann von Meyer, the greatest comparative anatomist that Germany has produced, recognized the fact that the extinct reptiles of the secondary strata have characters in common, which separate them from their living allies; and he proposed to name the group Palæosauria. He further defined this sub-class of the Reptilia as comprising three chief divisions; first, flying animals, now termed Ornithosauria or Pterodactyles; secondly, swimming animals, such as Ichthyosaurs and Plesiosaurs, which he named Nexipods; and thirdly, animals resembling the larger and heavier land mammals, which he called Pachypods. These divisions were at once generally accepted, though the name Pachypoda has given place to Dinosauria. It is impossible to over-estimate the philosophical merit of Von Meyer in dealing with these fossil remains, especially when their fragmentary nature is remembered; and also bearing in mind the slow growth of knowledge which has enabled us to confirm and appreciate the truth of his views concerning the Dinosauria.

Von Meyer's opportunities were almost limited to the study of fossils found in the Triassic rocks of Central Europe; and owing perhaps to the slow diffusion of scientific information, the somewhat similar remains which were met with in the Wealden strata of the south-east of England were studied without regard, either to the evidence furnished by German specimens, or to Von Meyer's generalization.

* This lecture was originally delivered on the 19th April, 1879, at the Scientific Club in Vienna. An abstract of it, rendered in German by Count Marschall, has already been issued in the *Monatsblätter des wissenschaftlichen Club in Wien.*

The great merit of Mantell's discoveries is not easily undervalued, when the large collections which he formed are studied in the British Museum; and the many memoirs in which he made their structures known enunciate conclusions which Professor Owen in most cases adopted when afterwards describing the Fossil Reptilia of Britain.

Mantell, however, affiliated the animals which he found to the Saurians, for in those days the order Sauria was often used to comprise both Lizards and Crocodiles, so that a blending of the characters of bones from both these groups of animals appeared in no way unnatural. But, probably, Cuvier's suggestion concerning the resemblance of the teeth of Iguanodon to the teeth of the living Iguana, and the knowledge that teeth among the higher group of mammals often furnish unfailing evidence of the skeletal characters of the animals to which they belong, especially influenced Mantell in regarding these fossil Saurians as little more than gigantic fossil Lizards. The wealth of materials furnished by the English strata for a long time drew to their study some of our ablest anatomists; but it was not till 1859 that Professor Owen proposed to separate them from Lizards, and instituted the order Dinosauria, which was chiefly defined by having more than two vertebræ in the sacrum. The Dinosauria are nothing more than Von Meyer's Pachypoda under a new name. Professor Owen, however, was hindered from fully appreciating the true nature and structure of these extinct animals by a few identifications of parts of the skeleton which have since been better elucidated. Thus, the animals were at first supposed to have long clavicles somewhat like those of Lizards, and the coracoid was also supposed to be in the main like that of a Lizard. The bones of the hind limb and pelvis were imperfectly known or understood; and it was not until Professor Cope, aided by beautiful specimens from the Greensand of New Jersey, studied the anatomy of Dinosaurs, that it began to be understood that many of the Dinosaurs had a kangaroo-like form, and that bones which were referred to the arch for the support of the fore-limb really belonged to the arch for the support of the hind-limb. Shortly afterwards Professor Huxley, in 1868, independently came to conclusions almost identical with those of Professor Cope; and, aided by the material discovered by Professor Phillips in the Forest-Marble near Oxford, was able to demonstrate that the supposed coracoid was the ilium, and the supposed clavicle was the ischium. There exists fortunately in the Oxford Museum a cast of the tibia and astragalus of Pœcilopleuron; and the character shown by these remains demonstrates that in this part of the skeleton Dinosaurs present the closest possible resemblance

to birds. Since the pelvis was bird-like, and the tibia and tarsus bird-like, and the early dorsal vertebræ were often convex in front and concave behind, like the later dorsal vertebræ of Penguins, Professor Huxley strongly supported Cope's conclusion that the Dinosauria made a near approach to birds. In later years Mr. Hulke, in England, has added largely to our knowledge of the Dinosaurian skeleton; while in America, Professors Marsh and Cope have disinterred and described gigantic Dinosaurs, which for beauty of preservation and completeness of the skeleton surpass all the remains previously found in Europe. These later writers have adopted the doctrine of avian affinities for the Dinosauria.

The whole of the Dinosauria, so far as is known at present, were land animals, and their remains are most abundant in those formations which give evidence of near proximity to land, such as the Trias, Wealden, and Greensand formations; though they are represented in almost all the other Secondary deposits. From indications of the characters of hind limb, vertebræ, and skull, I am inclined to believe that Protero-saurus, found in the Permian, and best known as the fossil Monitor of Thuringia, must be included in the Dinosaurian order. In the Trias, especially of Württemberg, as may be seen in the magnificent collection administered by Dr. Fraas at Stuttgart, and in the University Museum established by Professor Quenstedt at Tübingen, Dinosaurs are especially abundant. Perhaps the most interesting types are Zanclodon and its allies, some of which are still undescribed. In England the Bristol conglomerate and other Triassic beds have yielded the remains of Thecodontosaurus, Palæosaurus, and Teratosaurus. The Lias at Lyme Regis has yielded Scelido-saurus, and at least one other undescribed genus; while another genus appears to be indicated in the Lias of Elgin. Megalo-saurus, though especially characteristic of the Stonesfield Slate and Wealden beds, commenced, so far as can be judged from teeth, in the Lias, and continued, as Professor Suess has discovered, to the Upper Greensand. Ceteosaurus, if indeed that name can be retained, abounds in the Forest-Marble, Kim-meridge Clay, and Wealden beds. Cryptosaurus and Megalo-saurus are both found in the Oxford Clay; Priodontognathus probably belongs to the Calcareous grit, though it may be Wealden. The Dinosaurs of the Kimmeridge Clay include Omosaurus, Gigantosaurus, and some doubtful forms; the Wealden beds are characterized by Iguanodon, which, how-ever, ranges from the Kimmeridge Clay perhaps to the Upper Greensand. With it occur Hylæosaurus, Hypsilophodon, Pelorosaurus, Polacanthus, Vectisaurus, and Ornithopsis; while in the Upper Greensand of Cambridge, among other genera

are Macrurosaurus, Anoplosaurus, Syngonosaurus, Eucercosaurus, and Acanthopholis. The last genus also ranges into the Chalk. On the Continent, after the Trias, Dinosaurs are rarely found. Thaumatosaurus from the Inferior Oolite is a species of Pliosaurus, and has no Dinosaurian characters. The Continental Wealden specimens are as yet undescribed; but Professor Suess has found in the Gosau beds a rich Dinosaurian fauna, the general nature of which was briefly indicated by Dr. Bunzel. Professor Fritsch has figured a fragment of a Dinosaurian limb-bone from the Chalk of Bohemia, and long ago Dinosaurian bones were figured from the Maestricht beds, some of them closely conforming to the Iguanodont type. Nothing need be said now of the many American Dinosaurs described by Leidy, such as Cœlosaurus, Hadrosaurus, and Astrodon, which, together with Lælaps of Cope, occur in the Greensand of New Jersey; while in the Far West Professor Cope has found Agathaumus, Cionodon, and Polyonax. More recently there have been obtained from Colorado Camarasaurus of Cope, which appears to be identical with Atlantosaurus of Marsh, and is associated with genera named by the latter author Morosaurus, Apatosaurus, Allosaurus, and Diplodocus, and genera named by Cope Amphicœlias and Epanterias. It is, however, at present uncertain whether the latter genera are all distinct from the former.

This great succession of animals exhibits a diversity of organization exceeding so far as can be judged from the bones, that observed in any existing order of reptiles. It is probable that in the long period of geological time the group underwent evolution, not merely in the sense of the skeleton becoming modified by being adapted to altered conditions of existence, forced upon the animals by changing forms of the land-surfaces of the world, but also in actual progress from lower to a higher type. Yet, with all this diversity, there is rarely any difficulty in recognizing the distinctive Dinosaurian characteristics of the animals, as shown in the transverse platform of the neural arch from which the neural spine rises, in the form of the scapular arch, and the pelvic girdle, and the characteristic shapes of the principal bones of the fore and hind limbs. But when the Dinosaurs first appeared in the Trias, the difficulty of defining the limits of the order is great on account of the way in which many of the types approximate to Crocodiles and other lower animals. In fact, the Triassic Crocodilia seem to foreshadow the Dinosaurs, though the two groups are contemporaneous. This is evident from the circumstance that Professor Owen formed the order Thecodontia to receive such types as the Thecodontosaurus, which Professor Huxley has shown to possess the essential charac-

teristics of Dinosaurian structure; and that Professor Huxley has himself thought it necessary to institute under the name Parasuchia, a division of the Crocodilia further removed from living crocodiles than the Teleosauria of the secondary rocks, for such extinct genera as Belodon and Staganolepis, formerly classed as Thecodonts. Von Meyer at first referred Belodon to the Pachypoda, but afterwards thought that Belodon was more of a Crocodile than a Lizard, and could not be included in that group. Many of the remains at first referred to Belodon from the Stuttgart neighbourhood are really referable to Zanclodon; but the true Belodon presents characters which show a singular transition in some respects from the Crocodile towards the Dinosaur. Thus with a Crocodilian twist the long femur has more the shape of a Dinosaurian femur than that of the Crocodile, and it has the characteristic Dinosaurian trochanter fairly developed on the inner side of the shaft, though placed a little higher than usual. The humerus is, however, much more Crocodilian in having the radial crest reflected downward instead of inward; and the animal was covered with immense and close set plates of dermal armour, which rested upon the expanded tops of the neural spines, and this covering gave to it a very Crocodilian aspect. Similarly, the Aëtosaurus of Fraas, sheathed in armour of a like kind, yet has a remarkably Dinosaurian type of pelvis; and the femur, like that of Belodon, shows the trochanter on the middle of the shaft, which is generally regarded as Dinosaurian. The tarsus, however, is distinct, the astragalus and calcaneum being separate.

Now it is quite possible that these animals and Staganolepis are more nearly related to the Dinosaurs than to Crocodiles; but if so, it only demonstrates that some Dinosaurs must be much more Crocodilian than we have hitherto had reason to believe. There are several modifications of the Dinosaurian skull. One specimen figured by Mr. Hulke, and referred to Iguanodon, exhibits the form of the brain-case and its central cavity, showing that the brain was remarkable for its great height, that it was compressed from side to side, and apparently distinguished into cerebrum, optic lobes, and cerebellum, placed successively behind each other, on the reptile plan, though not like any living reptile, and making no approach in shape to the brain of a bird (see Fig. 2). A portion of the base of the brain-case of a Dinosaur from the Potton sands, which has been named Craterosaurus, is similar to the same region of the skull in the New-Zealand Lizard Hatteria, but makes several approximations to the Crocodilian pattern. Another Dinosaurian skull is referred to Scelidosaurus by Professor Owen; and skulls of the Hypsilophodon have been figured by Professor

Huxley and Mr. Hulke (see Fig. 1). Now when these skulls are critically compared with those referred to Belodon, some of which may belong to Zanclodon, the resemblance of plan is sufficiently close to arrest attention; and points in which triassic skulls differ from Crocodilia are points in which they approach the Dinosauria. If the Crocodilian skull were supposed to be plastic and squeezed together from side to side, so as to become much narrower, and sufficiently elevated to make the eyes nearly vertical, then the chief point necessary to complete the Dinosaurian resemblance would be great relative enlargement of the cerebral cavity and the temporal fossæ and a reduced length of the jaw. But if instead of comparing the Dinosaur with the living Crocodile, we had taken a representative of the fossil group of Crocodiles called Teleosauria, the resemblance would have been, with such modifications as suggested, almost perfect, nor would the palate have been so entirely incomparable with the Dinosaurian palate in certain Teleosaurs as in the existing Crocodile. These resemblances, however, are probably of value merely as showing that Dinosaurs and Teleosaurs may be descended from a common stock; for as the Teleosaurs are unknown in the primary rocks, there is no reason for inferring that the one group was evolved from the other. The palate in Dinosaurs, so far as may be judged from its condition in Scelidosaurus and Hypsilophodon, finds its nearest parallel at the present day in the New-Zealand lizard Hatteria, in which the pterygoid bones have a similar form and send processes outward and backward to meet and lap along the inner processes of the quadrate bones, which articulate with the lower jaw, are similarly separated in the median line, and unite with comparatively slender palatine bones. It is difficult to estimate the importance of this and other resemblances which Dinosaurs show in the skull to Hatteria, especially since, taken as a whole, they do not counterbalance the remarkable resemblances to the Crocodilian group. Perhaps they rather indicate that the systematic place of Hatteria is nearer to Crocodiles than its earlier location among the Lizards might have suggested. The anterior nares are always placed well forward (Fig. 1), divided and almost surrounded by the premaxillary bones, much as in the Alligator. The nasal bones and frontal bones are elongated, as among Crocodiles, but it is not quite clear at present whether the frontal bone always enters into the cavity for the orbit of the eye. The frontal and parietal together contribute to form the large lateral surfaces behind the eyes, to which were attached the muscles which worked the lower jaw. At first sight this region offers some resemblance to Hatteria. The teeth vary in kind in different Dinosaurs. Perhaps the most Crocodilian type is Mega-

losaurus, for certain living Crocodiles have the lateral ridges of the teeth marked with minute serrations. Professor Huxley has shown that all Dinosaurian teeth, no matter how worn ultimately, are characterized by more or less marked serrations of the compressed margin. The teeth in front are frequently simple and conical incisors, contained in the pre-maxillary bone, which do not become worn down, because they appear to have had adapted to them a toothless pad in the lower jaw, which was hollowed out something like the beak of a parrot. Behind these, without any intervening canine tooth, so far as known forms go, succeed the molars, which in the herbivorous genera usually have their surfaces worn more or less flat. The distinction into pre-molars and molars is a character fairly marked in the Crocodilian jaw, where the canine teeth also may be detected; though, owing to the molars not usually being worn down, they do not present the modified appearance seen in Dinosaurs. The resemblances perhaps are equally close in the teeth of Lizards, where pre-molars and molars are often easily recognized, though some Lizards, like the *Cnemidophorus lacertinoides* from Mexico, show distinct incisors, pre-molars, and molar teeth with cusps, like those of mammals. Among the Reptilia, probably no part of the skeleton is of less value than the teeth for purposes of classification; and the same type, if not the exact form, is maintained in nearly allied genera. Taking the skull as a whole, and contrasting its Triassic forms with those of the Wealden beds, the differences which have developed themselves are in all cases such as would be held, in the vertebrate province, to pertain to a higher group of animals; but though in Hypsilophodon the eyes rest against hollows in the frontal bones (Fig. 1), as among some birds and many mammals, there is nothing in the skull which indicates that the Dinosauria are a transitional group, passing into either the Mammalian or Avian Class.

The vertebral column shows a good deal of variety. If Professor Owen is correct in identifying as Dinosaurs certain reptiles from the Trias of South Africa, the vertebræ may have the articular faces of the centrums as deeply biconcave as in the living Hatteria or in Ichthyosaurus. In Zanclodon the cervical vertebræ are greatly elongated, like those of a long-necked ruminant, and concave at both ends (Fig. 4). In this genus, as in Morosaurus, figured by Professor Marsh, the atlas and axis are separate bones, as in the existing crocodile, the odontoid mass of the atlas fitting firmly, however, against the front of the second vertebra. This is the more interesting because in some Wealden types, like Iguanodon, the odontoid process is anchylosed with the axis, so as to closely approximate in appearance to the condition of the axis in a bird (see Fig. 3).

The Crocodilian resemblance of the bones in Zanclodon is the more remarkable because in the Teleosaurs there is no such Crocodilian condition of the earliest neck vertebræ, but a close resemblance to the same bones in Plesiosaurus. Here, then, as in the skull, there is evidence of evolution, in so far that in later time these joints of the neck acquire the characters seen in a higher organic type. The centrums or bodies of the vertebræ, both in the neck and back, in many, if not all, of the older Dinosauria, have their articular surfaces flattened or concave; and though this condition is retained in some of the latest known representatives of the group, such as the Dinosaurs of Gosau and of the Cambridge Greensand, many other genera, such as Megalosaurus and most of the Wealden types, have the bodies of the neck vertebræ convex in front and concavely-cupped behind, as among the ruminant mammals. This is the more interesting because the character was at first supposed to indicate a distinct genus, which was named Streptospondylus. It is probably one of the characters by which the Dinosauria will hereafter be divided into family groups. It is especially remarkable, because no reptile presents the same modification, though the neck is so curiously constructed among Chelonians as to present this condition in one or two vertebræ. This reptilian resemblance is at least as strong and as important as that seen in the lower dorsal vertebræ of penguins, where the centrum is also opisthocœlous; especially as the character becomes lost in the lower dorsal region of Dinosaurs. Consequently here, too, the change which the mode of union of the vertebræ presents is best explained as a condition of evolution going on within the group, by which, as ossification became more perfect, the neck vertebræ, like some of those of the back, put on a character that is only similarly developed among mammals. In all the Dinosauria the neck vertebræ have small, short ribs, with a double articulation, formed on the Crocodilian plan, (Fig. 4); and in the dorsal region the rib rises entirely on to the neural arch, and has two distinct articular facets, the head joining the side of the neural arch, and the tubercle being connected with the transverse process (Fig. 5). In the older genera the dorsal vertebræ are moderately compressed from side to side, but in the newer types they often become more compressed, and the articulations for the rib rise higher up the neural arch, indicating an increased development of the lungs. The transverse platform of the neural arch in the region of the back, which is one of the distinctive characters of a Dinosaur, has a very bird-like appearance; but it differs from that of the Crocodile chiefly in the transverse processes being relatively shorter, and lifted higher up the sides of the neural arch to make room for the lungs. No part of the

Dinosaurian skeleton is more characteristic than the sacrum (Fig. 6), which usually consists of a number of vertebræ blended together, so as to form a strong mass, to which short ribs were attached. These vertebræ are stated to be always more than two; perhaps there may be no limitation in number. Certainly in Zanclodon there are only two sacral vertebræ united together. Thecodontosaurus, according to Professor Huxley, had three sacral vertebræ; Hylæosaurus not fewer than four; Megalosaurus and Iguanodon five. In Anoplosaurus there are six, and in Agathaumus there are said to be eight or ten sacral vertebræ. Probably two is the primitive number, as among Crocodiles. And just as Professor Huxley has analysed the chick sacrum into four true sacral elements, with dorso-lumbar elements in front and caudal elements behind, which have become blended together owing to the extension along them of the bones of the pelvis which are known as the iliac bones, so the extension of the ilium backward and forward in Dinosaurs, beyond the limits of the two original vertebræ, has caused the sacrum to increase in length, and thus to approximate in the number of bones it includes to the sacrum of a bird. But if it is bird-like at one end of the series it is crocodilian at the other; and here also we have evidence of a remarkable change going on in the skeleton of successive genera in past time, which I am inclined to regard as entirely functional and correlated with the circumstance that the earlier triassic Dinosaurs progressed in the manner of Crocodiles, while some of the later forms put so much of the energy of movement into the hind-limbs that they acquired a more or less erect mode of progression. The sacrum of Anoplosaurus is especially remarkable for the fact that its neural canal is enlarged so as to be wider than the entire centrum of a dorsal vertebra.

The tail varies in length. *Zanclodon lævis* has thirty-seven caudal vertebræ; other Dinosaurs have probably many more. In the long-tailed genera, like Lælaps, Ceteosaurus, and Macrurosaurus, the later vertebræ become elongated into a dice-box shape. Perhaps the tail is shortest in Anoplosaurus from the Cambridge Greensand, in which only eight vertebræ have been found, and probably but few more existed. Many Dinosaurs, though not all, have in the first half of the tail V-shaped bones articulated at the junction of each pair of vertebræ on the underside. Sometimes these chevron bones (see Fig. 7) are long, at others short; sometimes the V-shape becomes changed into a triangle by the articular facets uniting, as among certain lizards; occasionally they are prolonged to the end of the tail. The tail shows less modification, perhaps, than any other part of the skeleton. As among Crocodiles the

transverse processes disappear in its hinder part, and the earlier caudal vertebræ are usually massive but short (Fig. 7). There is nothing about the tail suggestive of resemblance to a bird, and the characters of the caudal vertebræ of Archæopteryx are quite different, nor would it be worth while examining how far the tail may be paralleled among the Mammalia. It is, however, right to mention that in certain Dinosaurs, which are otherwise imperfectly known, the articular ends conform to the Crocodilian pattern in being procœlous.

When the elements which form the vertebral column are freed from the matrix so as to be adapted together by their natural surfaces, they usually fall into three marked curves. First the cervical vertebræ form a curve which is convex on the under side and concave above, showing that the neck was usually arched upward. The dorsal and sacral vertebræ similarly form a curve in the opposite direction, that is to say, convex in length on its upper side, between the fore and hind limbs, indicating that the back was arched. While the tail vertebræ similarly form a curve in the reverse direction, or concave on the upper margin, and its concavity is usually so well marked as only to be intelligible on the hypothesis that the root of the tail was elevated to a considerable height above the ground. In the larger-tailed Dinosauria the depth and thickness of the tail was immense, and perhaps entirely Crocodilian in the relative proportion which it bore to the depth of the body.

The ribs in the dorsal region appear to have been long, well curved, directed outwards, and sometimes upwards, so as to enclose a large visceral cavity. They are usually compressed from side to side, and sometimes flattened above, so that the cross-section of the upper part of a rib resembles a capital T. This character is especially seen in the Dinosaurs from the Greensand of England and Gosau; and, judging from the way in which the transversely expanded neural spine of Belodon is correlated with the support of dermal armour, it seems probable that lateral stripes of dermal armour may be inferred from this condition of the dorsal ribs, especially as massive dermal plates are found in association with the remains in both those localities. The specimen of Ceteosaurus at Oxford includes a bone which Professor Phillips interpreted as the sternum. Such a bone may perhaps exist in some of the German triassic genera, but no other trace of it has been met with, and but few ribs could have been articulated to it.

The scapular arch changes but little throughout the entire group (Fig. 9). It consists of a broad rounded or quadrate coracoid, which unites with the long and flattened scapula to form the glenoid cavity which receives the head of the humerus.

The coracoid is always pierced with a foramen near to its union with the scapula. The scapula always widens at its union with the coracoid, and sometimes, as in the cretaceous genera, developes a not inconsiderable acromial process, which is absent in examples from the older rocks. In Megalosaurus the scapula and coracoid are often blended together. In many genera the scapula has a slight backward curve, and is also convex in length, so as to adapt itself to the form of the ribs. This form of scapular arch finds no parallel in the form of the bones among existing Reptilia except in the Chamæleon and Hatteria. Yet the resemblance of these bones to some in the Triassic Crocodilia is remarkably close, and the absence of clavicles rather points to a Crocodilian than a Lacertilian affinity.

The fore-limb presents nothing bird-like (Fig. 9). The humerus, as already remarked, differs from that bone in the Crocodile rather in detail than in plan, and the expansion of the head of the bone and of the distal condyles, probably has more relation to the weight of the animal to be supported than to the characters of the type from which the group was modified. The head of the bone, however, is not parallel to its distal end, and there are characteristics that would as well admit of a Lacertilian as of a Crocodilian interpretation; yet the form of the bone in most cases permits no doubt as to the ordinal group to which it belongs. The ulna and radius were placed one behind the other, more after the manner of the Lizard, perhaps, than of the Crocodile. Specimens in the British Museum show that the radius was received in a groove or depression of the ulna at its proximal end. The olecranon process at the humeral end is large (Fig. 9). The carpus has hitherto been imperfectly figured. It is represented by Professor Owen in Iguanodon in union with the ulna and radius, and appears to consist of two bones. In this genus Professor Owen has figured a remarkable spur on the inner side of the foot, thus identifying a bone which Mantell formerly regarded as a nasal horn, and which Professor Owen had subsequently interpreted as a claw phalange. The specimen is obscure, but from evidence that many Dinosaurs have five digits in the fore-limb it may not perhaps be impossible that this supposed anomalous spur may be an abnormally modified digit. The number of phalanges in the fore-limbs appears to be variable. Altogether the fore-limb is but little specialized, and might have been modified from any generalized reptilian type. Its characters have been but little discussed. Taken as a whole, it must be regarded as furnishing important evidence of reptilian affinities, which, unlike the parts of the skeleton already mentioned, change their type but little in the various genera which conserve this grade of animal organization.

The pelvis, on the other hand, is unlike that of any reptile. As is well known, the Ilium, or bone which extends along the sacral vertebræ, is in Reptiles either vertical or directed backward, though it attains its most expanded form in the Crocodiles (Fig. 11). In Birds it extends about equally before and behind the acetabulum, or cup for the articulation of the femur. In the Mammals it is directed almost entirely in front of the acetabulum. Too much importance ought not to be attached to the direction of this bone as a mark of affinity, because its direction is almost always entirely forwards in frogs, probably without any other explanation than the influence of the manner of progression of the Anura in determining the direction of its growth. Similarly we are as little justified in regarding the Avian extension of the Dinosaurian ilium as a mark of Avian affinity in these animals, as we should be in regarding the direction of the Anurous ilium as an evidence of Mammalian affinity. When the shape of the ilium in Triassic Dinosaurs is examined and compared with that of the Crocodile, the resemblance is closer than might have been anticipated. The ilium has its chief extension backward; there is a process directed downward to the ischium, and forward to the pubis. This is well seen in Zanclodon, as is proved by specimens in the Tubingen collection; and I infer by analogy that the left ilium of Thecodontosaurus figured by Professor Huxley also had its chief extension behind the acetabulum (Fig. 12). But there is no doubt that other genera, like Iguanodon (Fig. 13), had the anterior process of the ilium, which is not developed in Thecodontosaurus, extended far forward; while other genera, like the Morosaurus of Marsh (Fig. 10), show the bone to have great depth, and, if rightly interpreted, to have its chief direction inclining anteriorly. There would thus appear to have been a progressive modification of this bone, by which a reptilian form which was associated with a Reptilian mode of progression became changed for an Avian shape in animals in which the progression was birdlike. And, although I confess to doubts concerning the right reading of some of Professor Marsh's figures, it would appear that these changes of form tended slightly towards the Mammalian pattern. The ischium is essentially a Crocodilian bone; and in the Atlantosaurus or Camarasaurus figured by Professor Marsh has a shape which is Crocodilian, meets the pubis much in the same manner as in the Crocodile, and the bones appear to have a Crocodilian direction. But, on the other hand, in some other genera, such as Iguanodon, ischium and pubis are slender bones, which resemble, both in form and direction, the same elements in the Apteryx or the Emeu; and this resemblance is the more interesting since Mr. Hulke has shown that the pubis developes

a large anterior pre-pubic process, which may be considered Lizard-like or Chelonian, but is not altogether unparalleled by the small pre-pubic process of the Apteryx. Professor Marsh has described more than one remarkable modification of this type of pelvis, which appears to show that a distinctive Reptilian feature is retained, even after the bones acquired the closest possible resemblance to Avian forms and manner of arrangement. The pre-pubic process, however, might be regarded as analogous with the pre-pubic bones of Ornithosaurus and Monotremes, although in those groups it is a distinct ossification.

The hind-limb offers a nearer resemblance to that part of the skeleton in a bird, than in any other animal, and is much more bird-like than the hind-limb in any existing reptile, but its Avian characteristics have probably been over-estimated (Fig. 10). The femur, or thigh-bone (Fig. 14), is long and strong, and readily recognized by three characters: first, the depth to which its head is received into the cavity in the ilium, which usually makes the whole of the upper, or proximal end of the bone an articular surface, and renders it impossible to define the head of the bone from its shaft, because there is no constriction between them; secondly, there is almost always a slender free process, or trochanter, directed upward on the outer front margin of the bone, which appears to correspond, in part at least, to the great trochanter; and, thirdly, on the inner border of the shaft of the bone, at, or above its middle part, is another trochanter, compressed from side to side, usually called the third, and regarded as peculiarly Dinosaurian, which is nothing but the lesser trochanter of the mammalian femur somewhat differently placed. This latter conclusion is satisfactorily demonstrated by remains of a new genus from the Upper Greensand of Gosau, placed in my hands by Professor Suess. Here a strong muscular crest, which may be exactly compared to the posterior ridge between the trochanters on the human femur, sweeps down in a curve from the outer and upper margin of the hinder side of the bone to the inner trochanter. But though the aspect of the femur thus becomes Mammalian, the bone is nevertheless modified from a Crocodilian plan. If we suppose the head of the Crocodilian femur to have the apex of its triangular outline on the middle of the inner margin immensely enlarged at the expense of the anterior part, it becomes Dinosaurian. And on the middle of the inner side of the shaft of the bone the Crocodile has a trochanteroid thickening, which corresponds with the inner or 'little' trochanter of Dinosaurs; and just as in the Gosau specimens, an 'inter-trochanteric' muscular ridge extends from it upward and

outward to the proximal end of the bone. Both the trochanters are sometimes wanting in American genera. In Belodon, the femur diverged but little from the shape in living Crocodiles, but its slight modifications are all in the Dinosaurian direction; and it appears to me, that, like so many other of the elements of the skeleton, the femur underwent evolution in successive genera, which caused its original somewhat Crocodilian appearance afterwards to approximate to the shape that characterizes the bone in Birds and Mammals.

The next segment of the hind-limb, consisting of the tibia and fibula, is sometimes much more Avian in its character, but differs fundamentally from this part of the bird's skeleton in having the fibula better developed, so that it extends down to articulate with the tarsus, while in birds it tapers away to a needle point, and never is inserted into the tarsus at all. The Avian character is, therefore, to be found in the tibia only. And here it is limited to two points. First, the development of a strong anterior crest, which is directed forward and outward, so as often to extend in front of the fibula, is a Dinosaurian character met with, to some extent, among Birds, but quite as well marked in Mammals. Secondly, the shape of the distal end of the bone is bird-like; but that form is found in no adult bird, and is only to be detected in the young bird before the tarsus has become blended with it; so that while it might go to show, perhaps, that Birds are descended from a common stock with Dinosaurs, it would be misleading to regard it as altogether Avian, since the character is lost in the adult bird's skeleton. It is only in such genera as Cœlosaurus (Fig. 16), Megalosaurus, and Iguanodon, that the tibia has a shape that suggests the bird; while in Hylæosaurus (Fig. 15), it is, if not Crocodilian, more suggestive of a crocodile than a bird; and, so far as can be inferred from figures of the Colorado type named Morosaurus (Fig. 10), the tibia in that genus is altogether Reptilian. But when the tarsus, or rather the astragalus, is closely applied to the tibia, as in Megalosaurus, Pœcilopleuron, Lælaps, or Iguanodon, it gives the bone a resemblance to the bird which is almost convincing, since the parallel extends to nearly every detail. The character, however, is shorn of much of its importance, when we remember that there are many Dinosaurs in which there is an os calcis, or heel-bone, placed side by side with the astragalus (Fig. 17); and that in other genera like Morosaurus, the astragalus was not an Avian conformation, so that it is not a general characteristic of the order, and, therefore, must have undergone change in the group. It seems, hence, more reasonable to infer that the Avian type of the bone is more likely to be a consequence of the hind-limbs being used for progression

in certain genera, after the manner of birds, than that what is among existing animals a class-characteristic, should disappear, especially as Morosaurus is one of those genera in which some of the vital organs made a remarkable approximation to the bird-type. Morosaurus had two rows of tarsal bones. The metatarsals are three, as in Laosaurus, four, as in Scelidosaurus, or five in number, as in Morosaurus; but they are never anchylosed into one mass, as in Birds. The number of phalanges increases in the outer digits as in Birds and Reptiles. The claw phalanges are sometimes compressed from side to side as in Birds, sometimes from above downward, as in Reptiles.

This survey of the osteology of the Dinosauria leads us, I think, to conclude that whatever the group eventually became, it originated in a close family relationship with the Crocodilia; and that plan, which was never entirely obliterated, underwent a development in most parts of the skeleton, more perfect than is known to have occurred in any other group of animals. But this series of changes in the skeleton did not convert the crocodile into a bird or a mammal, or make it anything but a near ally of the crocodiles, which, owing to the influence of conditions of existence, came to simulate birds in some parts of the skeleton and mammals in other parts. But it is impossible to believe that these modifications of the hard parts of the animal went on without the soft vital organs changing also; and all analogy would suggest that the change was progressive, so that the vital organs of the Crocodile may have greatly altered. But whether the progress extended so far that certain Dinosaurs became eventually changed into birds in all essentials of organization and in some parts of the skeleton, while others approximated to mammals, is a speculative problem, attractive in its plausible simplicity, with which facts do not yet enable us to deal in a satisfactory way; though it must be admitted, they give some support to such an hypothesis.

A satisfactory classification of the Dinosauria can hardly be made until we know far more of the fossil representatives of the group, more of the extinct reptiles of South Africa, of the Labyrinthodontia, of the Ichthyosauria, and of the Ornithosauria, for with all these animals Dinosaurs show some evidences of affinity. The higher Labyrinthodonts, which, in some unexplained reverence for formulæ, have got themselves included in the Amphibia notwithstanding the characters of their skull and axial skeleton, certainly closely approach Crocodiles, and therefore Dinosaurs and Ichthyosaurs. The Ichthyosaurs are perhaps such a group as the Dinosaurs might have become under aquatic conditions of existence; for although the resemblances of Ichthyosaurs to ordinary Dino-

saurs are of the most slender kind, their resemblances to the triassic Crocodilia are more important, and suggest such a view as a possibility.

Professor Cope divided the Dinosauria into three groups. The first, Symphypoda, comprises Compsognathus from the Solenhofen Slate, and Ornithotarsus from the New Jersey Greensand. The second group, Goniopoda, in which the tarsal bones embrace the tibia, includes Megalosaurus and Iguanodon. While the third group, called Orthopoda, is represented by Scelidosaurus, and comprises Dinosaurs in which the proximal tarsal bones are separate, and do not embrace the tibia. Professor Huxley has disputed the value of this division, and believing Compsognathus to be a type outside the limits of the Dinosauria, has united the two groups together under the name Ornithoscelida. But seeing that the proximal tarsal bones are united with the tibia after the manner of birds in some Ornithosaurs, while in other genera they always remain separate, it seems probable that the Dinosauria might naturally include Compsognathus. Professor Huxley has subdivided ordinary Dinosaurs into three families, distinguished by their teeth and other characters. They are named from typical genera Megalosauridæ, Scelidosauridæ, and Iguanodontidæ. But though these are the best marked Dinosaurian groups in England, there are many Dinosaurs that it would be difficult to place with these. Lately, Professor Marsh proposed a new sub-order, Sauropoda, for the gigantic Dinosaurs described by himself and Professor Cope from Wyoming and Colorado. I may perhaps be permitted to state that on very slender evidence I ventured to predict the existence of this group nearly ten years ago. It happened that I was studying Dinosaurs, and arrived at views about the skull which led me to examine a bone in the British Museum, long previously regarded as the tympanic or quadrate bone of Iguanodon. It appeared to be an imperfect dorsal vertebra, and as there was another and earlier vertebra in the collection of a like character, I named the form Ornithopsis Hulkii, in honour of Mr. Hulke. The genus was named Ornithopsis on account of a bird-likeness, in nothing more remarkable than in the way in which the bodies of the vertebræ were excavated, as in Birds and Pterodactyles for the reception of an air-cell prolonged from the lung. Professor Cope has shown by his admirable descriptions that this is the most remarkable character of the great Colorado Dinosaurs, such as Camarasaurus, which differs from Ornithopsis in minor generic characters. The type is truly Dinosaurian, and therefore profoundly interesting as showing that animals which put on so many avian characters in their bones, acquired lungs only comparable in complexity and plan to those of Ptero-

dactyles and Birds. So that we may say that this group which I had formerly in mind when using the name Ceteosauria demonstrates that certain Dinosaurs approximate to both those groups, and may have possessed the vital organization of a bird. Hence Dinosaurs are to Crocodiles what Birds are said by evolutionists to be to Lizards.

Thus I have endeavoured to bring together not merely strings of names of animals in which the beautiful museums of Germany and Vienna are rich, but have tried, by a touch of the scientific wand, to make them unfold their structure and history, and live again, not as mere dry bones, but clothed with a power of engendering truths.

EXPLANATION OF PLATE II.

FIG. 1. Right side of a generalized Dinosauriam skull, restored chiefly from a figure of Hypsilophodon, by Professor Huxley, and partly from Scelidosaurus and other materials.

FIG. 2. Outline of the brain-cavity of Iguanodon in the middle-line of the skull, after Mr. Hulke; the large size and upward extension of the optic lobes is more remarkable than in Teleosaurus, but the form of the brain-cavity is most like that of Ichthyosaurus.

FIG. 3. Axis of Iguanodon, from the Wealden Beds, showing the odontoid process and the two tubercles for the rib.

FIG. 4. Early cervical vertebra, with cervical rib of Zanclodon, from the Trias of Württemberg.

FIG. 5. Dorsal vertebra of Iguanodon, showing that the attachments of the rib are entirely on the neural arch.

FIG. 6. Sacrum of Iguanodon, seen from the visceral surface, consisting of five vertebræ, anchylosed together.

FIG. 7. Mid-caudal vertebra of Iguanodon, showing the long neural spine and large chevron bone.

FIG. 8. Centrum of vertebra of Ornithopsis, showing the cavity for the air-cell which excavates the body of the vertebra.

FIG. 9. Fore-limb of Morosaurus, after Professor Marsh.

FIG. 10. Hind limb and pelvis of Morosaurus, after Professor Marsh.

FIG. 11. Ilium of Alligator. *a. p.* The anterior process.

FIG. 12. Ilium of Thecodontosaurus, placed for comparison with the Alligator. *a. p.* The anterior process.

FIG. 13. Ilium of Iguanodon, after Owen and Huxley. The Ilium of Megolosaurus is intermediate between this type and that of Morosaurus, shown in fig. 10. *a. p.* The anterior process.

FIG. 14. Femur of Iguanodon, after Professor Owen.

FIG. 15. Tibia of Hylæosaurus, after Mr. Hulke.

FIG. 16. Tibia of Cœlosaurus, after Professor Leidy.

FIG. 17. Distal end of Tibia and Fibula in Ornithotarsus, after Professor Cope, showing their union with the astragalus and calcaneum.

REVIEWS.

PALÆONTOLOGY.*

PROFESSOR NICHOLSON has published so many text-books and manuals on various subjects of Natural History, that in a very short time, if he continues in the same fashion, his works of this kind will make a small library of themselves. His latest production is a second edition of his *Manual of Palæontology*, the first issue of which took place just seven years ago, and this has been so thoroughly revised and so greatly augmented that, as he himself tells us in his preface, it may be considered practically a new book, although of course in the general treatment of the subject the author has followed much the same lines that were laid down in his former edition. The last section of the first edition, treating of Palæontology from a historical or stratigraphical point of view, has been expanded by the author into a separate book, having the title of *The Ancient Life-History of the Earth*, in which the subject is developed in much more detail than could be compatible with the plan of the present work, and hence the author has thought it best to omit this section altogether. Thus without diminishing the usefulness of his book, he has been enabled to avail himself of a considerable amount of space for the more complete exposition of his primary subject.

This consists in a regular, systematic treatment of the fossil forms hitherto discovered in the strata of the Earth's crust, and it must be confessed that Dr. Nicholson has been most successful in carrying it out. Commencing with the Sponges, as the lowest types, he proceeds upwards through the various classes of the animal kingdom, the fossil remains of animals naturally occupying the greater part of the work; and in all cases the descriptions of the fossil forms are preceded by a short introductory essay on the characters of the different groups as manifested at the present day, a most important element in the philosophical conception of the subject, as too many palæontologists are inclined to forget that in the interpretation of fossils they must be guided solely by considerations drawn from existing species.

* *A Manual of Palæontology for the use of Students, with a General Introduction on the Principles of Palæontology.* By Henry Alleyne Nicholson, M.D., &c. Second edition, 2 vols. 8vo. Edinburgh and London: Blackwood and Sons. 1879.

Palæobotany, or the palæontology of plants, is treated of rather cursorily in the last few chapters of the book; and there, also, the author has changed his plan. Instead of attempting to give a systematic revision of the fossil forms of plants, he has employed the chronological mode of treatment, which perhaps, considering all things, is the best that can be adopted for conveying a general idea of this department of the subject. The fact is, that from our imperfect knowledge of fossil plants and the very nature of those organisms, it would be almost impossible to produce a good picture of this department of palæontology on a systematic basis within moderate limits, and the interest attaching to the geological history of plants is generally derived, less from the consideration of the individual types than of their combinations to form Floras, which must necessarily be treated in connection with the strata in which they occur.

It would be useless, and indeed most uninteresting, to the reader, if we were to attempt anything in the shape of an analysis of the chapters of Dr. Nicholson's work devoted to palæozoology. It will be sufficient to say, in general terms, that the book has evidently been prepared with the greatest care, and that, on testing various parts of it, we find that the author has embodied in his statements the results of nearly all recent researches in all the departments of the subject. This is especially observable in the second volume, which chiefly deals with the Vertebrata; here we find that some of the latest contributions to the history of extinct reptiles, birds, and mammals, have been made use of by the author. In his preface, indeed, he tells us that owing to some delay in publication, after the preparation of the greater part of the book, and indeed after a considerable portion of it was in print, he has been unable to avail himself of certain important memoirs which have appeared in the interim, such as Zittel's valuable systematic works on Fossil Sponges, Professor Möbius' treatise on *Eozoon canadense*, Mr. Moseley's researches on the Stylasteridæ, and several others which have appeared chiefly in the course of the year 1879. Such causes of regret, however, are almost inseparable from the production of a work of this magnitude; and students of palæontology have every cause to be grateful to Dr. Nicholson for the labour which he has expended in the elaboration of this Hand-book.*

With the view of rendering his book as complete as possible, the author prefixes to the systematic part of it, a general introduction, intended to

* Although we have thought it unnecessary to attempt any criticism of Dr. Nicholson's work, we may call attention to the fact that Dr. Traquair has written to various periodicals, calling attention to a misstatement of his unpublished views as to the systematic position of certain Fishes (Vol. II., pp. 134 and 138). Dr. Nicholson has in consequence sent out a statement, which may be pasted into the book as an erratum, to the effect that, 'The reader will kindly delete "Platysomidæ" from the list of *sub-orders* of Ganoids, and place these fishes next after the Palæoniscidæ as a *family* of Lepidosteidæ. . . . . Dr. Traquair holds that the Platysomidæ constitute a *family* very distinct from the Pycnodontidæ, but closely allied to Palæoniscidæ, and that the position of both Palæoniscidæ and Platysomidæ is rather in the sub-order Acipenseroidei.'

'Dr. Nicholson's mistake,' says Dr. Traquair, 'is evidently due to his having had only a very hurried glance over my proof-sheets, and that only on a single occasion.'

explain to the reader the conditions under which fossils are presented to his consideration, and the consequences to be drawn from their study. In successive chapters he deals with the nature of fossils and rocks, the characters of the sedimentary rocks, their chronological succession, and the application of palæontology to the elucidation of the latter, the causes of imperfections in the palæontological record, and the conclusions that may be drawn from fossils, winding up with a chapter on the classification of the animal kingdom and the succession and progression of organic types. All these subjects, many of which are rather difficult to handle, are well treated; and the student who masters these chapters will approach his practical studies with a better intellectual foundation than can be boasted of by many professed palæontologists. A full index and a good glossary of terms employed will add greatly to the usefulness of the book.

We have yet to say a few words upon the way in which these volumes are got up. In type and paper they leave nothing to be desired, and the woodcut illustrations, most of which are original, and the work of Mr. Charles Berjeau, are really beautifully executed. The book is in fact almost what our neighbours across the Channel would call *un ouvrage de luxe*. But we fear that these very characters, which cannot but excite our admiration and approval, must raise the cost of the work to an extent which will practically cut it off from the use of the great majority of the students for whose behoof it is said to have been prepared.

TABULATE CORALS.*

IN the preface to his *Manual of Palæontology*, Professor Nicholson remarks that he would have considerably modified the section dealing with the so-called 'Tabulate Corals,' had his investigations into the structure and relationships of these fossils been completed in time. The result of these researches is now before us in the form of a magnificent octavo volume, which we have no hesitation in pronouncing the best of all the author's numerous works.

The group of Tabulate Corals (*Madreporaria Tabulata*) was founded by MM. Milne-Edwards and Haime in the introduction to their splendid monograph of the British Fossil Corals, published by the Palæontographical Society in 1850. It was established for the reception of a number of rather problematical stony structures, ranging in time from the Silurian period to the present day, and exhibiting a considerable diversity of character, but agreeing in having well-developed walls, and the visceral cavities of the individual polypes divided into a series of chambers, one above the other, by complete diaphragms or transverse floors. Added to these characters was the complete absence, or rudimentary condition of the septal system, which was never represented by anything more than a few 'trabeculæ' projecting into the visceral chambers. The principal living forms referred to this

* *On the Structure and Affinities of the Tabulate Corals of the Palæozoic Period, with critical descriptions of Illustrative Species.* By H. Alleyne Nicholson, M.D., &c. 8vo. London and Edinburgh: Blackwood and Sons. 1879.

group by M. Milne-Edwards, belonged to the genera *Millepora*, *Heliopora* (representing the family Milleporidæ), *Pocillopora* (referred to the Favositidæ), and *Seriatopora* (the type of the Seriatoporidæ); and the fossil forms were more or less closely associated with these in accordance with the resemblances apparently presented by the respective polyparies. The great majority of the fossils were from palæozoic deposits, and the well-known forms of *Favosites* and *Halysites* may serve as typical examples of the group.

Although MM. Milne-Edwards and Haime established the section Madreporaria Tabulata for the convenience of having some group to which to refer a considerable series of fossils with which they were obliged to deal, it is very clear that they felt by no means convinced that it was a sound zoological group; and as early as the year 1860, we find M. Milne-Edwards himself, in the third volume of his *Histoire Naturelle des Coralliaires* (p. 223), remarking that 'according to recent observations made by M. Agassiz upon the Millepores, it is probable that it will have to undergo great alterations, or perhaps be placed entirely in another class of the animal kingdom.' This passage alludes to the discovery by Agassiz of the Hydrozoal nature of *Millepora*, which has since been amply confirmed by Mr. Moseley during the voyage of the *Challenger*, and by Professor P. Martin Duncan from the researches made many years ago by General Nelson at Bermuda. Further investigations have shown that notwithstanding the correctness of Professor Agassiz's observations, the removal of all the Tabulate Corals to the Hydrozoa, as suggested by him and supported by some other naturalists, would have been a mistake, the recent genus *Pocillopora*, especially, proving to be a true Zoantharian allied to the well-known Oculinidæ. The morphological importance of the 'tabulæ,' or transverse floors, the presence of which constituted the chief bond of union between the members of the group, was also shown to have been overrated, as similar structures occur in a good many genera of undoubted Zoantharian corals. The combined researches of palæontologists in fact showed pretty clearly that the group would never be removed 'en entier,' as suggested by M. Milne-Edwards, to any class of the animal kingdom.

Professor Nicholson enters in considerable detail into the history of the section Tabulata, which he shows clearly must be 'broken up and undergo redistribution.' The performance of this task is, however, a matter of no small difficulty, the great majority of the species referred to in this group being old Palæozoic fossils, with no very distinct relationships to any existing forms. Our author is inclined to recognize about twelve distinct groups of these corals, the affinities of which he discusses (pp. 12–29). The Milleporidæ, here greatly reduced in compass, must, as we have seen, be referred to the Hydrozoa in the vicinity of *Hydractinia;* and the Pocilloporidæ have been shown by Professor Verrill's researches upon the animal of *Pocillopora* to be Zoantharians allied to the Oculinidæ. *Seriatopora*, a recent coral, and with Milne-Edwards the type of a distinct family, is regarded as so closely related to *Pocillopora*, that it must also be transferred to the same neighbourhood. The Favositidæ (Honey-comb Corals) which constitute by far the greater part of the section as proposed by Milne-Edwards, are pretty generally regarded as true Zoantharia, to which the Columnariadæ and Syringoporidæ also probably belong, although their precise position is much more

doubtful. *Heliopora*, the type of the Helioporidæ, has been proved by Mr. Moseley to belong not to the Zoantharia, but to the Alcyonaria, and it carries with it the fossil *Heliolites*, with which, on the other hand, the 'Chain Corals' (Halysitidæ), the Tetradiidæ, and the Theciidæ, and, perhaps, also a portion of the Chætetidæ, are considered by Professor Nicholson to be nearly allied, so that the members of these groups will probably have to be placed among the Alcyonarian polypes, or occupy a position on the boundary line between the two great groups of Actinozoa. The curious Silurian genus, *Labechia*, has apparently baffled all the efforts of our author to give it a 'local habitation' in the system. The genus *Aulopora* and its allies, constituting Milne-Edwards' sub-order of Zoantharia Tubulosa, are noticed by Professor Nicholson as having been considered to be founded upon young colonies of *Syringopora*, an opinion in which he does not share; but he abstains from assigning them to any definite place in the system.

It will be seen from the preceding statements that our author is inclined altogether to abolish the Tabulate Corals as a group; and further, that in some cases he is not inclined to accept the family groups proposed by his predecessors. The portion of his book, following the summary of general systematic results, from which the foregoing particulars have been derived, is devoted to the detailed description of the Palæozoic genera and many species constituting the different groups formerly referred to the sub-order. It is impossible to overlook the many signs of the most careful work displayed in this portion of Dr. Nicholson's book; and although, as we have seen, he is compelled by the very nature of the objects of his study to leave many points in a state of uncertainty, which, indeed, may perhaps never be cleared up, he may certainly 'lay the flattering unction to his soul' that in his present volume he has made our knowledge of these puzzling organisms take a great step forward.

In the matter of illustrations, also, we are here in a condition of magnificent luxury. Scattered through the text are numerous woodcuts, some of them the same as those illustrating the chapter on Fossil Corals in the author's *Manual*, others original and prepared specially for the present work, but all excellent. In addition to these, we have fifteen beautifully executed plates, exhibiting magnified sections and other details of the Corals described. The figures on these plates, like those on wood, are all from the author's drawings, and reproduced by Mr. Berjeau, upon whom they certainly reflect the greatest credit.

SIBERIA.*

IN *Frozen Asia* we have an extremely interesting account of a country with which most of us are entirely unacquainted; and Mr. Eden has supplied a brief, but in its way very complete sketch of this territory. After giving in the first two chapters some account of the geographical

* *Frozen Asia; a Sketch of Modern Siberia; Together with an Account of the Native Tribes inhabiting that Region.* By Charles H. Eden, F.R.G.S. 8vo. London. Society for Promoting Christian Knowledge. 1879.

features of the country, in the third he presents us with a short summary of the Geology of Siberia, for which, however, he states that he is indebted to Mr. W. W. Waddington, of Brazenose College. To this succeeds a chapter on Mineralogy, which might with advantage have been extended to greater length, considering the vast mineral resources of the country, but as these resources are far from being developed, it would perhaps be hardly fair to complain of the author's brevity in this instance. We may, however, remind the author, that malachite (p. 55) is carbonate and not oxide of copper. The chapter on the Fauna of Siberia is of particular interest, the account of the discovery of remains of the mammoth especially being compiled with no little skill. Curiously enough, the fur-bearing character of Siberia is scarcely mentioned, and the sable does not even make its appearance in the list of Siberian mammals. Of the Flora we hear very little. Perhaps there is little to tell, except that 'enough wheat could easily be raised to supply the entire European market,' a fact which may be thought worthy of notice at the present time. Only two pages are devoted to climate, of the rigour of which we gain some idea from the fact that at Yakutsk, 'the coldest city in the world,' the mercury 'remains congealed for two, and sometimes three months in the year.' Concerning the native races and 'folk-lore,' we have upwards of a hundred pages. This part of the book is perhaps the most interesting in the volume, and though but a compilation from the writings of other authors, contains many facts which must prove new to the general reader. The volume concludes with an account of recent explorations down to the time of Professor Nordenskiöld's latest expedition, and is illustrated with a map of Siberia, very nicely executed, by Stanford. The book is in every particular a very pleasant and readable one, and deserves to meet with favour at the hands of the public.

METALLIFEROUS MINERALS AND MINING.*

THE author states in his preface that it has been his design to describe in a concise and systematic manner the conditions under which metals and metalliferous ores occur in various parts of the world. He also expresses a hope that such descriptions may serve, in the first place, to explain, to some extent, the origin of ore deposits, and, secondly, by defining the zones occupied by the ores of the different metals, to lessen the amount of risk with which metalliferous mining is invariably attended. He further hopes that the figures and other data which he furnishes may have the effect of more clearly defining the conditions resulting in successful mining enterprise.

The volume, which extends over nearly 450 octavo pages, is illustrated with numerous woodcuts, for the drawings for which the author expresses his obligations to his son; and although among these we recognize some

* *A Treatise on Metalliferous Minerals and Mining.* By D. C. Davies, F.G.S., Mining Engineer, Examiner of Mines and Collieries, Member of the Geologists' Association of London, Honorary Member of the Natural Science Society of Chester and Wrexham, &c. Author of a *Treatise on Slate and Slate Quarrying*, &c. London: Lockwood & Co., 1880.

with which we have long been familiar, they are, as a whole, not only well drawn, but also judiciously selected for the purposes of illustration.

There is throughout this work evidence of much patient compilation, and it undoubtedly contains a considerable amount of useful information. It is, however, to be regretted that sufficient care has not been exercised in the exclusion of error, and that when generalization has been attempted it becomes obvious that the author does not possess a sufficient grasp of the subjects of which he treats.

A list of such inaccuracies would exceed the limits of our space, but a few only of those which have been observed may be cited as examples.

It is stated (p. 79) that 'all the gold-bearing rocks lie below the Carboniferous group,' whereas it is well known that the majority of the rocks of the gold regions of California are Jurassic.

We are informed (p. 113) that 'the presence of silver in lodes or strata is not observed in beds newer than the Cambrian limestone.' All who possess a competent acquaintance with such subjects are, however, aware that the Great Comstock Lode of Nevada, which since its discovery some twenty years ago, has produced silver to the value of 40,000,000*l.* is enclosed within walls of undoubted Tertiary age, and that the vein itself is probably not older than the Pliocene period.

With regard to lead ore it is stated (p. 236) that its highest horizon 'is found in the Carboniferous Limestone, this horizon being divisible into two zones; one in the pale-coloured massive limestones near the base of the series, and the other in the limestones and grits near the summit.' The annual production of 40,000 tons of lead ore from the Bunter sandstone of the Düren and Commern districts in Germany is certainly a somewhat remarkable exception to this generalization.

We are taught (p. 280) that 'the geological horizon of the ores of mercury ranges from the summit of the Lower Cambrian rocks to the base of the Llandeilo beds of the Cambro-Silurian strata.' Is Mr. Davies aware that the highly productive quicksilver mine of New Almaden in California, and several others in that country, occur in rocks of Cretaceous age?

Turning from the distribution of metalliferous minerals to the statistics of their production, we still find inaccuracies, but space permits us to mention only two of them. At p. 121 the annual production of copper in Spain is stated to be about 6000 tons, whereas two Spanish mines alone produced above 20,000 tons of that metal during the year 1877. Again (p. 197) the annual production of lead in Belgium is estimated at 700 tons; reliable statistics show that the production of that country in 1876 amounted to no less than 6973 tons of metallic lead.

It will be seen from the following observations on the rocks of Cornwall, that many of the opinions brought forward differ materially from those accepted by geologists. At p. 128 when speaking of these rocks the author states:—'In maps they are all included as Devonian, which is, I think, with all deference to other observers, a mistake. My own interpretation of the ages of the strata I have just described is this:—The granitic masses belong to the Laurentian or the base of the Cambrian, the hornblende slates with the serpentine, and porphyries, to the Cambro-Silurian, the

argillaceous slates to the Silurian, passing gradually in their upper portion into the Devonian.'

In going through this volume, one cannot but sympathize with its author, who states (p. 397) that he feels ashamed when he thinks 'how German mining engineers may read the reports of some of our mine captains and experts on mineral properties.' We would add that we are by no means satisfied that the opinion of foreigners, with regard to English mining literature, will be materially improved by a perusal of the book before us. It is, however, probable that the information vouchsafed (p. 417), namely, that 'potash is potassium mixed with varying proportions of other substances,' will be new to them.

ROUGH WAYS MADE SMOOTH.*

THIS is another of the volumes of interesting essays for which Mr. Proctor is becoming famous, there being few authors gifted with his wondrous power of making interesting and instructive articles out of some of the most difficult branches of science. Like most of the preceding volumes, *Rough Ways made Smooth*, is essentially a collection of some of the numerous articles contributed by him to the magazines during the last few years. We note, however, that many of them have been revised and amended so as to render them more suitable for their present purpose. The present volume commences with an essay on the 'Sun's Corona and his Spots,' in which the author shows that the theory that the corona varies in size directly as the number of spots, is without adequate basis, although several of those who have devoted much attention to this subject were led to suppose that it did. The second essay is on 'Sun-spots and Commercial Panics,' and is devoted to showing the fallacy of the idea that sun-spots rule the commercial morality of civilized nations. The idea of a possible connection between sun-spots and commercial panics is founded on one of those partial coincidences which are so fascinating to the minds of the quasi-scientific, or to even *savants*, when they sally out of their own domains into less familiar regions.

The third essay, 'New Planets near the Sun,' deals in a popular manner with the observations of Professors Watson and Swift during the eclipse of July 1878. The fifth and sixth essays deal with the Moon, and have special reference to its past history and the supposed new crater. There are one or two errors in these, not so much on the part of the author, as on the part of those whose opinions are quoted. The early history of the Moon, like the early history of the Earth, must be discussed by one having the combined resources of chemistry and mathematics at his command. Any discussion by one who is purely a chemist, or purely a mathematician, must inevitably be vitiated by errors due to an imperfect grasp of the chemical or mathematical side of the problem. In several places, Professor Newcomb's opinion is quoted on selenographical subjects, although the experience of that eminent astronomer must be most limited in these matters. Essays seven and eight

* *Rough Ways made Smooth.* By Richard A. Proctor. 8vo. London: Chatto and Windus. 1880.

deal with the meteors of the great shower of November, and those which seem associated with the orbit of Biela's comet. The meteor-shower which it was anticipated would be seen when the Earth passed nearest to the orbit of the comet, was not seen, either in November 1878 or November—December, 1879.

The remainder of the volume is devoted to essays of less strictly scientific nature, but in most cases of most interesting character. Those on 'Artificial Somnambulism,' 'Hereditary Traits,' and 'Mechanical Chess-players,' are especially well worth reading. On the whole, like most of Mr. Proctor's books, it is one which is far more easily taken up than laid down.

ERASMUS DARWIN.*

THE student of English books dating back some thirty or forty years and upwards has doubtless often met with fine if rather magniloquent verses quoted from a work called the *Botanic Garden.* This was a didactic poem of no small merit, in whatever light we look at it. Published originally in 1790 and 1791, its author was Dr. Erasmus Darwin, of Lichfield, a man of considerable repute in his day, and who would have occupied a far higher niche in the temple of Fame if his contemporaries had been in the least degree capable of appreciating his qualities. As most naturalists have long been aware, this Dr. Darwin was the originator of a conception of organized nature on the basis of evolution, which met with but contemptuous treatment from the author's countrymen. He has also a claim on our gratitude as having been the grandfather of Charles Darwin, whose influence upon the progress of Natural History has probably been greater than that of any other writer since the days of Linnæus.

A short note on his grandfather's biological views inserted by Mr. Darwin in the later editions of the *Origin of Species* excited the curiosity of a German *savant*, Dr. Ernst Krause, to know something more of the opinions entertained by Erasmus Darwin; and the result of a careful study of the writings of the latter on the part of Dr. Krause, was the publication in *Kosmos* of February last of a biographical and critical essay, a translation of the latter part of which, preceded by a biographical notice from his own hand, is now given to the public by Mr. Darwin.

Dr. Krause speaks of Erasmus Darwin as follows:—'This man, equally eminent as philanthropist, physician, naturalist, philosopher, and poet, is far less known and valued by posterity than he deserves, in comparison with other persons who occupy a similar rank. It is true that what is perhaps the most important of his many-sided endowments, namely, his broad view of the philosophy of nature, was not intelligible to his contemporaries; it is only now, after the lapse of a hundred years, that by the labours of one of his descendants we are in a position to estimate at its true value the wonderful perceptivity, amounting almost to divination, that he displayed in the

* *Erasmus Darwin.* By Ernst Krause. Translated from the German by W. S. Dallas. With a preliminary notice by Charles Darwin. Small 8vo. London, John Murray, 1879.

domain of biology. For in him we find the same indefatigable spirit of research, and almost the same biological tendency, as in his grandson; and we might, not without justice, assert that the latter has succeeded to an intellectual inheritance, and carried out a programme sketched forth and left behind by his grandfather.'

'But, at the same time, we remark a material difference in their interpretation of nature. The elder Darwin was a Lamarckian, or, more properly, Jean Lamarck was a Darwinian of the older school, for he has only carried out further the ideas of Erasmus Darwin, and it is to Darwin therefore that the credit is due of having first established a complete system of the theory of evolution.' Clearly such a man as this deserves to be rescued from the comparative oblivion into which he had fallen, and we are glad to see that two writers have in this same year tried to do justice to this English worthy.

It is hardly necessary to say that the life of Erasmus Darwin as a successful physician in a provincial town passed without any striking incidents. What is to be told, however, is told here by Mr. Darwin in an exceedingly pleasant style, and he naturally devotes his attention rather to the delineation of his grandfather's character than to the thankless task of giving full details of a somewhat uneventful life. Nevertheless, the few incidents narrated, and especially the quotations given from letters, are full of interest for those who like to realize as far as possible the modes of life and thought prevalent at a time so near our own, and yet so widely separated from us.

Erasmus Darwin was descended from a family of landed gentry in Lincolnshire, and was born on the 12th of December, 1731, at Elston Hall, in Nottinghamshire. He early displayed a great fondness for poetry, and also for mechanical pursuits, both of which tastes he preserved to the end of his life; and when very young, according to his elder brother, Robert, he used to show little experiments in electricity with a rude apparatus he then invented with a bottle. In 1750 he was entered at St. John's College, Cambridge, and took a respectable degree in that university in 1754. In the autumn of the latter year he went to Edinburgh to study medicine, returned to Cambridge in 1755 to take his degree of Bachelor of Medicine, then went again to Edinburgh, whence he returned in September, 1756, and settled as a physician in Nottingham. Being unsuccessful in that city, he removed in two or three months to Lichfield, where, owing to the reputation made for him by two or three successful cases, he speedily got into good practice. At Lichfield he remained for twenty-five years, and then removed to Derby, where he died on April 18, 1802. Dr. Darwin was twice married, first to a Miss Howard, whom he lost after thirteen years of married life, and secondly to a widow lady, for whom he appears to have entertained a strong passion even during the life of her first husband, and who survived him.

Of his grandfather's character Mr. Darwin speaks very unreservedly, but on the whole, sums up, and justly, in his favour. As he says, 'There is, perhaps, no safer test of a man's real character than that of his long-continued friendship with good and able men.' Darwin's intimate and almost life-long friends were such men as Josiah Wedgwood, Keir, the chemist, Day, the author of *Sandford and Merton* (are any of the rising generation, we wonder, acquainted with Harry, and Tommy, and that wonderful Mr. Barlow?),

Boulton, and Watt, the engineers, and Mr. Edgeworth. He appears to have been incessantly active, kindly, although sometimes sarcastic, liberal in his practice, and very free from what is usually considered as one of the weaknesses of a poet, vanity. His feelings towards his children have been represented by his female biographer, Miss Seward, as not of the most amiable character; and some of his letters to them show a curious coldness: but Mr. Darwin proves clearly, indeed from that lady's own admission, that there was no truth in the most repulsive of her stories, and he accounts for her ready acceptance of so much that told to the disadvantage of Dr. Darwin, by the motive of disappointed affection, the lady having, it appears, manifested a strong desire to marry the doctor after the decease of his first wife. After his death she probably thought that a little spite could do him no harm, while it would certainly be a stab for her successful rival; and it would appear that Mr. Darwin's father was in possession of documents connected with the doctor's relations with Miss Seward, which he thought it would be unpleasant for her to have published.

What Mr. Darwin modestly calls his *Preliminary Notice* to the translation from Dr. Krause's essay occupies about three-fifths of the little volume that he has published in commemoration of his grandfather, and we fear that we have followed his example in the present notice. We, have, however, already indicated in general terms, borrowed from Dr. Krause's essay, what was the character of Dr. Darwin's philosophical work, and the essay on his scientific labours is even in the original so condensed that we could hardly do justice to it without transferring the greater part to our pages. 'Almost every single work of the younger Darwin,' says Dr. Krause, 'may be paralleled by at least a chapter in the works of his ancestor: the mystery of heredity, adaptation, the protective arrangements of animals and plants, sexual selection, insectivorous plants, and the analysis of the emotions and sociological impulses, nay, even the studies on infants are to be found already discussed in the writings of the elder Darwin.' It is to be remarked, however, that Dr. Darwin nowhere gives his ideas on the nature and evolution of organisms in a connected form, but the arguments and details are scattered profusely through the notes appended to his various didactic poems, and only partially brought together more closely in some parts of his great prose work, the *Zoonomia.* But even in these scattered notices we recognize, as indeed is remarked by Dr. Krause, the most wonderful resemblances between the elder Darwin and his still more distinguished descendant. In both we see the same philosophical breadth of view, combined with the same extraordinary power of grasping and bringing together an immense mass of details from the most varied sources to support and illustrate the argument under discussion—in fact, the same qualities which most strike us in Mr. Darwin's work are the most prominent characteristics of his grandfather's, and their exercise led both to approximately the same result. That is to say, Erasmus Darwin, equally with Charles Darwin, arrived evidently at a belief in the origin of the diversity of organized forms by a process of evolution one from the other, but the former regarded this process as brought about by internal impulses, at least semi-conscious, rather than by the action of external conditions giving rise to a process of natural selection under the struggle for existence.

EVOLUTION, OLD AND NEW.*

DR. KRAUSE, in the concluding paragraph of his essay on the works of Erasmus Darwin, says of his system that 'to wish to revive it at the present day, as has been actually seriously attempted, shows a weakness of thought and a mental anachronism which no one can envy.' This anachronism has been committed by Mr. Samuel Butler in a very interesting little volume now before us, and it is doubtless to this, which appeared while his own work was in progress, that Dr. Krause alludes in the above passage.

It is curious to observe how, when a new idea gains the predominance in any department of science, the opinions of men oscillate for a time, usually swinging, like a pendulum, to a greater distance in the new direction in proportion as they have previously been pulled in the other. Thus for many years, as we all know, the idea of the transmutation of animals and plants, by any means whatever, was regarded as most unorthodox, and scarcely a naturalist could be found who would admit that such a thing was possible. Nowadays, since Mr. Darwin's well-known works have shown that such a thing is not only possible, but that in all probability it has taken place, and in fact that on no other assumption can we account scientifically for the facts observed in nature, we find nearly all transformists pinning their faith exclusively to the theory of 'Natural Selection,' and indeed, to a very great extent, especially in Germany, to that extreme form of the doctrine of which Professor Häckel is the prophet. Under these circumstances, it is well that, from time to time, naturalists should be reminded that there are matters upon which the theory of Natural Selection, admirably worked out as it is, leaves us in the dark; that there is room for some diversity of opinion as to the extent to which the innate qualities of the organism may govern its evolution; and that we have not yet arrived at the point at which a new systematic philosophy of the universe may be proclaimed as the only truth.

In his present work (which, after the fashion of musical composers, he somewhat affectedly characterizes as 'Op. 4.') Mr. Samuel Butler takes up the writings of Buffon, Erasmus Darwin, and Lamarck, and subjects them to a searching investigation, with the purpose of showing that the fundamental idea of the origin of species advocated by these writers is identical. This, we think, he does fairly enough, except in the case of Buffon, whose statements, even upon Mr. Butler's own showing, vacillate in a most remarkable manner. Our author endeavours to account for this by ascribing to Buffon an 'ironical' intention in many of the expressions used by him in discussing questions bearing upon the origin of organic forms by descent with modification,—that is to say, that Buffon, having, by some of the statements made in his earlier volumes, 'got across,' to use a vulgar phrase, with the ecclesiastical authorities, afterwards took the precaution of hinting what he really thought, or stating it as a possible result of a certain way of thinking,

* *Evolution, Old and New; or the Theories of Buffon, Dr. Erasmus Darwin, and Lamarck, as compared with that of Mr. Charles Darwin.* By Samuel Butler. Small 8vo. London: Hardwicke and Bogue. 1879.

and then shirking the difficulty which was staring him in the face by winding up his remarks after an orthodox fashion. From what we know of Buffon's writings, we are more inclined to think that he really never arrived at any clear result in his own mind upon the subject of the possible genetic relationships of species of animals, and that what he says on the subject in the passages referred to, is in each case the expression of the passing opinion entertained by him at the moment. If this is the case, he can hardly claim to be the founder of the doctrine of evolution, although it is quite possible that some of his remarks may have assisted Erasmus Darwin to arrive at his much juster and profounder views on the subject. That Darwin was the evolutionary parent of Lamarck, seems almost certain. Mr. Butler here does full justice to the merits of both these great naturalists. We may remark that his analyses of their opinions are preceded by sketches of their lives, which will be read with interest; but we must warn the reader that, in the case of Darwin, the biographical information is mainly derived from Miss Seward's work, which has been shown not to be particularly trustworthy.

Mr. Butler points out clearly that the great difference between the old and new schemes of evolution consists in the assumption, in the former, that the variations of the organism are brought about by a sense of need on its part, no doubt in consequence of changes in its surroundings, but into his further considerations on the nature of this governing impulse, we shall not follow him. We may, however, recommend Mr. Butler's little volume to our readers, as one the perusal of which will open up to them some new views on the subject of Evolution.

THE LAST ARCTIC EXPEDITION.*

WE are glad to be able to announce that Captain Markham's little narrative of his experiences of Arctic exploration when on board the *Alert*, in the late expedition under Captain Nares, has reached a fourth edition. He gives an excellent, popularly written, and lively account of the expedition, and although the description of its scientific results makes no part of his design, he could not avoid, even if he tried to do so, giving many details of matters of scientific interest. At this festive season, when most of us have to exercise our ingenuity in selecting suitable presents for the youngsters, we have particular satisfaction in calling our readers' attention to Captain Markham's pleasant and nicely illustrated little book.

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* *The Great Frozen Sea; a Personal Narrative of the Voyage of the Alert during the Arctic Expedition of* 1875–6. By Captain Albert Hastings Markham, R.N. Fourth edition. Sm. 8vo. London: C. Kegan Paul and Co. 1880.

BRITISH PLANTS.*

WE learn from the Preface to this little volume that the illustrated edition of Mr. Bentham's *Handbook of the British Flora* has been exhausted, and we presume that, probably for good reasons, the publishers have no present intention of bringing out a new one. Under these circumstances they have decided upon printing separately the beautiful and characteristic series of small woodcuts of British Flowering plants and Ferns, drawn by those excellent botanical artists, MM. Fitch and Worthington Smith, and issuing them in a small volume at a low price. New figures of the species admitted into the British Flora since the publication of the illustrated *Handbook* have been added, and the little book may thus be used as illustrative of the later editions of Mr. Bentham's, or of any other Flora. To facilitate reference in the latter case, the index, which follows the nomenclature of the original work, includes a good many synonyms indicated by Italic type. The figures themselves are too well known to need any words of commendation from us.

PHYSICS.†

THIS is a very convenient and useful little book, which may be of service both to pupil and teacher. Its object is simply described in the preface: 'If schoolboys' notes could be taken down satisfactorily, and always found when wanted, there would be little need of this book. It is intended to form the basis of a course of lessons, illustrated by experiments according to the requirements of the class and the resources of the teacher, and it may be supposed to represent the notes—somewhat expanded—which the teacher would desire the class to take down and learn.' The Notes are contained in rather more than a hundred pages, and comprise plain statements with simple formulæ upon Sound, Light, Heat, Magnetism, and Frictional and Voltaic Electricity. An Appendix of about fifty pages is devoted to examination papers, with references to the paragraphs in which the solution of each question occurs. The writer of this notice has found the little work extremely handy as a skeleton form from which to lecture, and still more as a basis on which to found a brief *resumé* of the preceding, with which it appears to him judicious to commence each subsequent lecture.

W. H. STONE.

* *Illustrations of the British Flora; a series of wood-engravings, with dissections, of British Plants.* Drawn by W. H. Fitch and W. G. Smith. Sm. 8vo. London: L. Reeve and Co. 1880.

† *Lecture Notes on Physics.* By Charles Bird, B.A., F.R.A.S., Second Master in the Bradford Grammar School. London: Simpkin, Marshall & Co. Small 8vo, pp. 178.

UNITS AND PHYSICAL CONSTANTS.*

THIS book,' says the compiler in his preface, 'is substantially a new edition of my *Illustrations of the C. G. S. System of Units,* published in 1875 by the Physical Society of London, supplemented by an extensive collection of physical data. The title has been changed with the view of rendering it more generally intelligible. Though the publication is no longer officially connected with the Physical Society, the present enlarged edition is issued with the Society's full consent and approval.' In the first issue it was stated that its main object was to facilitate the *quantitative* study of Physics, and especially of the relations between its different branches. One uniform scale, a luxury hitherto unknown to the scientific calculator, is thus for the first time presented to the observer. The work is divided into eleven chapters, the first two of which deal with the general theory of Units, and the choice of three representing Mass, Length, and Time respectively. These are then applied to Mechanics, to Hydrostatics, to Strain, Stress, and Resilience, a new term introduced to remedy the ambiguity of 'Elasticity,' to Astronomy, to the velocity of Sound and of Light, to Heat, Magnetism, and Electricity. An appendix contains the Reports of the Royal Association Committee on Dynamical and Electrical Units, of which Professor Everitt was reporter.

The question of uniformity and the particular fitness for the purpose of the units chosen are too large for discussion in a mere notice of the book; but, granting its premises, there can be no doubt that the work has been thoroughly and judiciously carried out. W. H. STONE.

MODERN CHROMATICS.†

THE author of this very interesting work states his object to be to present, in a clear, logical, and, if possible, attractive form, the fundamental facts connected with our perception of colour, so far as they are at present known, or as they concern the general and artistic reader. For the explanation of these facts, the theory of Thomas Young, as modified and set forth by Helmholtz and Maxwell, has been consistently adhered to. It has also been his endeavour to present in a simple and comprehensible manner the underlying facts upon which the artistic use of colour necessarily depends. The book commences with a general statement as to the Transmission and Reflection of Light, in which it is shown that as painted or stained glass transmits enormously more light than pigments reflect in a properly lighted room, it follows that the worker on glass has at his disposal a much more extensive

* *Units and Physical Constants.* By J. D. Everitt, F.R.S., &c., Professor of Natural Philosophy in Queen's College, Belfast. London: Macmillan. Crown 8vo, pp. 175.

† *Modern Chromatics, with Applications to Arts and Industry.* By Ogden N. Rood, Professor of Physics in Columbia College. 8vo. pp. 330, with 130 Illustrations. London: C. Kegan Paul & Co. (International Scientific Series.)

scale of light and shade than the painter in oils or water-colours. In speaking, next of colour-production by dispersion, the use of the prism is explained, and the infinite number of gradations into which white light can thus be broken up; the positions of the coloured spaces, and the proportion occupied by each, is estimated according to original observations,—blue and blue-violet representing 311; violet, 194; red, 149; green, and yellow, and yellowish-green, 104; green and blue-green, 103; and all the others far smaller areas, raising the total to 1000 parts. The irrationality of the prismatic spectrum is contrasted with the normal length as obtained by means of a diffraction-grating, red here standing for 330 parts, violet-blue for 117, orange-red for 104, blue for 74, and the rest for the same complement; yellow here occupying the centre of the spectrum, the red and orange being considerably increased to the detriment of the blue and violet. It seems that the eye is far more sensitive to changes of wave-length in the middle regions than at either extremity. This is considered to negative theories of colour founded on supposed analogies with music.

The Constants of Colour are next adverted to, and the mode of mixing white light with the colours of the spectrum described. Most coloured surfaces are shown to be more or less adulterated with white light, and in many instances the latter even preponderates. The first Constant is therefore *purity*, or absence of white. The second is a photometric problem, namely, relative brightness, or *luminosity*. Here an ingenious system of rotating discs is brought into play, in which mixtures of colour in the form of sectors are estimated against proportional combinations of simple black and white. Black can hardly be looked on as an absolute negation, since it reflects from two to six per cent. Allowing for this, if white paper be represented by a luminosity of 100, chrome yellow has one of 80·3, emerald green of 48·6, cobalt blue of 35·4, vermilion of 25·7, and ultramarine of 7·6. The *hue* of colour is the third and last Constant; this depending on wave-length and position in the normal spectrum. The production of Colour by Interference and Polarization occupies the fourth chapter, the latter never being seen out of the laboratory, while the former furnishes the brilliant plumage of the peacock, the humming-bird, and the *iridescent armour* of beetles. The colours of *opalescent media* are next studied, as seen in milky solutions, in smoke, and, on the grandest scale, in the sky.

Fluorescence and Phosphorescence are next discussed, and the more essentially artistic topic of Colour by Absorption is reached. To this process the colours of ordinary objects are almost entirely due. This is studied by the spectroscope in tinted glass, and diagrams are given, showing the very complex nature of the waves transmitted by red, orange, green, and blue specimens. A hand-spectroscope suffices to demonstrate the same law in blue paper, which, besides white light, giving a continuous spectrum, reflects red, yellow, blue, and violet. The white reflected does not, in a strong colour, exceed that just named as emanating even from black. In velvet, an attempt is made to suppress all surface light, and to display only that which has penetrated deeply and thus become highly coloured. The green of vegetation offers a peculiar case. Extreme red is present, with orange, yellow, and yellowish-green, running into a full green. A mixture of red and green is, later on, shown to furnish yellow light.

A chapter on Colour-blindness is rather alien to the present notice. Helmholtz and Young's colour theory is reached in the eleventh chapter, and Maxwell's colour discs are explained. Overlapping spectra in Helmholtz's hands proved the singular fact that union of pure blue with pure yellow light, produces, not green, but white, thus overthrowing the hypothesis of Brewster. A section follows on the Mixture of Colours, founded mainly on Maxwell's Rotating Colour-discs and Dove's Dichroscope, the results of which are compared with mixtures on the artist's palette. Binocular Vision is afterwards called in aid, and many valuable practical results are recorded. The book, indeed, from this point, becomes less physical, but none the less useful, from the predominance of æsthetic detail. It would be impossible in limited space to analyze minutely the last chapters; but their general scope may be gathered from their successive headings:—Systems of Colours; Contrast; Combinations in Pairs and Triads; and the Use of Colour in Painting and Decoration.

It will be easily seen from what has been hastily sketched above, that this book is of exceptional value and originality; chiefly because it succeeds in blending harmoniously the latest researches in Physical Optics with the rules and dicta which are the hereditary property and the traditional foundation of Pictorial and Decorative Art.

W. H. STONE.

SCIENTIFIC SUMMARY.

ASTRONOMY.

Mars.—At the November meeting of the Royal Astronomical Society, Professor J. C. Adams read an interesting paper on the motion of the nodes of the satellites of Mars, and the important information which the observations of this motion would throw on the figure and constitution of the planet. Assuming that the planet was a symmetrical spherical body, Mr. Marth had shown, in the *Astronomischen Nachrichten*, No. 2280, that the nodes of the orbits of the satellites would have an annual motion, due to the Sun's action, amounting to 0°·06 in the case of *Phobos*, the inner satellite, and to 0°·24 in the case of *Deimos*, the outer satellite. If, then, the satellites of *Mars* preserved a constant inclination to the orbit of *Mars*, as would be the case were the planet a symmetrical spherical body, from the difference in the rate of motion of their nodes, they would in course of time have their orbits very much inclined to one another. So much so, that though at present the two satellites move in nearly the same plane, the plane coinciding with the plane of the equator of *Mars*, yet a thousand years hence they would move in orbits whose planes were inclined to each other by an angle of nearly 49°. Professor Adams draws attention to the fact that, if the planet *Mars* be not spherical but spheroidal, like our Earth, the effect of this deviation from a sphere would be to tend to make the satellites move in orbits very nearly coincident with the plane of the equator of *Mars*, and in this case the planes of the orbits of the satellite would always be nearly coincident, as they are known to be at present. As Laplace has shown, if the motion of the node due to the effect of the ellipticity of the planet be much greater than that due to the action of the Sun, the orbit of the satellites will always nearly coincide with the plane of the equator of the planet. Professor Adams now shows that, even if the planet be supposed to be only very slightly spheroidal, the effect of the ellipticity will very much exceed that of the Sun, so that the satellites will always move in orbits but slightly inclined to the plane of the equator of the planet. Now the effect of the ellipticity of the planet cannot be well determined from the motion of the node, because, owing to the slight inclination of the orbits, it will be difficult to accurately determine the motion of the nodes from observation. But the orbits of both satellites seem to be sensibly eccentric, and it will not be difficult to determine from

observation the motion of the apse or point where the satellite approaches closest to the planet; and from this motion it will be possible to accurately determine the ellipticity of the planet, and in this manner from the observed inclination of the orbit of the satellite, ascertain with accuracy the position of the equator of *Mars* and the figure of the planet. It is unnecessary to point out the great interest attaching to the exact determination of the position of the equator, and consequently of the poles, of *Mars*. There is some reason to believe from observation that the white polar caps of the planet, which are generally held to be due to the presence of arctic ice and snow, are not concentrically situated round the poles of the planet. It would throw material light on the physical constitution of the planet if this point were definitely settled.

The Secular Acceleration of the Mean Motion of the Moon.—The gradual quickening of the motion of the Moon in its revolution around the Earth has long been one of the most interesting points in the theory of the solar system. The comparison of the places assigned to the Moon by theory and observation seems to show that the observed acceleration is much greater than that assigned by theory. In consequence it has been supposed that a portion of the apparent acceleration may be really due to a slow retardation of the rotation of the Earth from the friction of the tides of the ocean against the crust of the Earth. Lately the researches of Professor Newcomb on the early observations of the Moon would seem to show that when the tables of the Moon are properly corrected, there is far less discordance between the observed and theoretical values of the acceleration of the Moon's motion than has hitherto been supposed. Lately Mr. J. N. Stockwell, in the *American Journal of Science*, November, 1879, has drawn attention to a new origin of a secular acceleration in the mean motion of the Moon. It has long been known that the attraction of a spheroidal body like the Earth on a distant body like the Moon is slightly different from that of a sphere of the same mass, the attraction being greatest when the satellite is in the plane of the equator and smallest when in the plane passing through the poles. For this reason a satellite revolving around the Earth in an orbit inclined to the equator would move slower than if it were moving in an orbit which was not inclined to the equator. It is known that at present the inclination of the lunar orbit to the terrestrial equator is very slowly decreasing, and it follows, therefore, that the Moon is gradually quickening in its motion round the Earth. Hitherto, however, it has been supposed that this acceleration was so small that it could not attain, even in the course of many centuries, to an amount which could be detected by observation. According to Mr. Stockwell, this is not so, as from his calculation he makes the acceleration from this cause amount to 0″·1981 per century. The amount of the acceleration varies as the square of the number of centuries, so that if this calculation be correct, it will be necessary to alter, by nearly five minutes, the theoretical time of the occurrence of the famous early eclipses of the Moon observed in Babylonia, and quoted by Ptolemy in his *Almagest*.

A New Planetary Nebula.—On November 14, 1879, while the Rev. T. W. Webb, F.R.A.S., the well-known astronomer, was sweeping the heavens near the constellation *Cygnus*, with his 9¼-inch reflector and a low-power eye-piece, he noticed an object resembling a 9th magnitude star, which

seemed to differ in appearance from similar stars. This led him to examine it with higher powers, when this object was seen to be not really a star, but to show an ill-defined bright disc, about 4″ in diameter, and of a bluish colour. When closely examined it seemed to be surrounded by a faintish glow. From Mr. Webb's description this object must have appeared similar to the planet *Uranus* when seen through a white, hazy mist. On referring to the star catalogue it was found to be the object observed as a star by Argeländer during his survey of the Northern Heavens, and recorded by him as No. 4004 Zone + 41°. Its place for the year 1880 is R.A. $21^h\ 2^m\cdot5$ Decl. + 41° 45′·3. Mr. Webb communicated an account of his observation to Dr. Copeland at Lord Lindsay's observatory at Dun Echt, in Aberdeenshire, who took the first opportunity of observing this object with the fine 15-inch refractor belonging to the observatory. According to Dr. Copeland this object is not quite circular, and has a sharp nucleus near its north preceding edge, and a faint diffusion of light on its opposite direction. It is, therefore, an exceptionally compact and bright planetary nebula. When examined with the spectroscope it gives three bright lines, one far brighter than the others, and these bright lines agree well in position with those found by D'Arrest to be generally given by nebulæ of this description.

Solar Parallax from Meridian Observations of Mars.—In the 'Observatory,' for November, 1879, Mr. A. M. W. Downing, of the Royal Observatory, Greenwich, has calculated the value of the solar parallax which results from the meridian observations of *Mars* made in the summer of 1877 at the observatories at Leyden and at Melbourne. Comparisons were only made on days when *Mars* and the comparison stars were observed at Melbourne, either on the same day as at Leyden, or on the following day. Twenty-nine corresponding sets of observations were obtained, and from them the solar parallax was computed. The separate results range between 8″·65 and 9″·39, and when properly combined, give as the final result:—

$$\text{Mean solar parallax} = 8''\cdot960 \pm 0''\cdot051.$$

Mr. Downing remarks, 'In the light of recent researches on the subject, we must, I suppose, consider this value of the solar parallax to be too large; it is, however, to be remarked that Professor Winnecke obtained the value 8″·96, and Mr. Stone that of 8″·94, from meridian observations of *Mars* in 1862; and though Professor Newcomb's discussion gave the result 8″·855, still, as Mr. Stone has pointed out, the smaller value depends chiefly on the use of the Santiago observations, which he declares to be inadmissible. It appears, therefore, that from meridian observations of *Mars* we get a parallax of not less than 8″·9; and we are almost forced to the conclusion that there is something in the *method*, which persistently tends to give a large result; if this be so it introduces another very perplexing element into researches on solar parallax.'

On examining the separate results given by Mr. Downing, it would appear that there is some trace of systematic error in the observations or in their reduction. Thus, as the result of the fourteen separate determinations deduced from observations made before opposition, we have a solar parallax of approximately 9″·087, whilst as the result of the fifteen separate determinations deduced from observations made after opposition, we have a solar parallax of

only 8″·917. This difference of 0″·120 is too large. The chief objection to this process is great liability to systematic error, not only from personality, but unless great care be taken, from errors in the correction for refraction, as well as in the assumed variation of the tabular error in declination. In the absence of any details it is impossible to say how far the result obtained by Mr. Downing may be affected by systematic errors of this nature.

The November Meteors.—In this year, as in the preceding years, the number of meteors belonging to the great system of *Leonids*, which have been observed, have been very few. The meteor-shower of November 12–14, is now reduced to the merest phantom of the great showers of the year 1866 and immediately succeeding years. At the Royal Observatory, Greenwich, the observation lasted for about four hours on each of the nights November 12, 13, 14, the circumstances being very favourable, the night being clear and there being no moon. The number of meteors seen were only 13, 19, and 32, on each night respectively, and of these only 6, 9, and 18, respectively belonged to the great system radiating from the constellation *Leo*. On November 12 and 13 watch was also kept by Mr. Corder, at Writtle, near Chelmsford; but only a very few *Leonids* were observed. Mr. W. F. Denning, of Bristol, also kept a close watch, with a similar result. On the 13th he observed a short, but very decided outburst of fine *Leonids;* but it soon died away. This short shower consisted of eighteen fine meteors, swift, but some leaving vivid bright trains. At Bedford Mr. T. P. Gray kept watch with a very similar result. We shall have to wait probably for years for another great display of these meteors.

Jupiter.—During the past summer great interest has been manifested in observation of the planet *Jupiter*, mainly on account of the presence on the southern bright zone of a long, narrow, elliptical spot, of a dark reddish colour. This spot seems to have first attracted general notice in the early part of the summer of 1879, though there is some reason to believe that it was visible as a very faint object in the preceding year. During the early part of the year the spot seems to have been lighter in colour than the great equatorial belt, which was brownish-red in hue. According to the measures of Messrs. Gledhill and Neisten, the spot was in south latitude 20° (Jovian), and in length covered about 33° of Jovian longitude. Its breadth was about 6° of Jovian latitude. Later on both the red spot and the great equatorial belt appreciably deepened in tint, and the elliptical spot is generally described by observers as being of the same hue as the equatorial belt, only more intense. During November it appeared to have slightly decreased in size, and to have moved a little nearer the equator. M. Neisten, of the Royal Observatory of Brussels, who has carefully watched the spot since June 25, 1879, is of opinion that it is surrounded by a bright elliptical ring, a ring brilliant white in hue, and much whiter than the bright zone on which the spot is situated. Other observers have seen a similar appearance. Mr. John Brett is of opinion that this bright ring is probably merely due to the effect of contrast. The Rev. T. W. Webb has noticed of late a haziness in the appearance of this spot, and describes it as only a little redder than the cinnamon-coloured equatorial belt. He describes the spot as having somewhat the appearance of two tufts of reddish wool touching each other. From comparisons which have been made, the conclusion has been drawn that this

peculiar red spot is slowly moving around *Jupiter*—an interesting point which ought to be carefully investigated.

Another feature, which has been very striking during the last opposition of *Jupiter*, is the common occurrence of round or oval, bright white spots. So bright and circular have these objects been that they could easily be taken for satellites in transit. These spots have been long known to occur. Some time back Mr. Brett pointed out that they were bordered on one side by shades, as if they actually cast shadows. Of late, however, it has been noticed that these shadings occur on both sides of the spots, and even all round, throwing them up, as it were, into relief. Under these circumstances, it is evident that these dusky borders cannot be shadows cast by the white spots, as supposed by Mr. Brett. Mr. Corder has concluded from his observations that these white spots are moving much quicker than the great red spot, as he is of opinion that he has been able to see one of these spots gain half a revolution of the planet on the red spot during a period of only twenty days.

Satellites of Mars.—The outer satellite of *Mars*, *Deimos*, was observed by Mr. A. Common, of Ealing, with his fine 37-inch reflector, on September 21st, 1879, more than three weeks before it was anticipated it could be seen with even the great Washington Equatorial. Later Mr. Common succeeded in observing the inner satellite, *Phobos*; and since then the two satellites of *Mars* have been regularly observed and measured at the Ealing Observatory. At Washington the outer satellite, *Deimos*, was seen on October 10th, the very day it was anticipated that it would come within reach of the great 26-inch equatorial. Since October 12 both satellites have been regularly observed by Professor Asaph Hall, at Washington. A measure of the position of *Deimos* was also obtained by Mr. Maunder, with the 12¾-inch equatorial of the Royal Observatory at Greenwich. Several observations have also been obtained at the Paris Observatory. At Dun Echt Observatory, Aberdeen, Lord Lindsay saw the first satellite with the 15-inch equatorial, but clouds prevented its being measured. Mr. Common is of opinion that any one who can see *Enceladus*, the second satellite of *Saturn*, ought to be able to see with ease both satellites of *Mars*, if only the precaution be taken to hide the planet behind a bar. He is of opinion that the outer satellite, *Deimos*, is quite as bright as *Enceladus*. *Phobos*, though harder to see from its proximity to the planet, is known to be far brighter than *Deimos*, probably as bright as *Tethys*.

By comparing the observations of the two satellites with the ephemeris founded on the observations made during 1877, Professor Asaph Hall finds that *Phobos* comes to its elongation $44^{m}\cdot0$ before the predicted times, the error in areocentric longitude of his ephemeris being $+ 34^{\circ}\cdot3$, so that the periodic time ought to be reduced by $1^{s}\cdot074$, and would be $7^{h}\ 39^{m}\ 13^{s}\cdot966$. In the case of *Deimos*, the outer satellite, the error of the ephemeris is very small. This correction will bring the value of the mass of *Mars* derived from the inner satellite into better agreement with the mass of *Mars* as determined from the motion of the outer satellite.

CHEMISTRY.

On the New Element, Scandium.—Clève has studied the properties of the new earth, Scandia, discovered by him a few weeks after Nilson's announcement, to which we drew attention (*Pop. Sc. Rev.*, Oct. 1879, 424), in gadolinite and yttrotitanite. The former mineral contains 0·002 to 0·003, and the latter 0·005 per cent of scandium. Scandia has the formula $Sc_2 O_3$, the ammonium- and potassium-scandium sulphates, as well as the oxalates and selenites, establishing this point. From some eight to ten grammes of scandia by repeated decomposition of its nitrate, one gramme of a white earth was obtained. This was converted into a sulphate and calcined, when 1·451 gramme gave 0·5293 gramme of scandia, which yields the number 44·91 as that of the atomic weight of scandium. If scandia be taken to be Sc O, the result given above would point to 45·94 as the molecular weight of the newly-found element, differing materially from 105·83, the minimum value, as found by Nilson. Careful examination with the spectroscope, by Thalén, proved Clève's scandia to be pure; it is inferred, therefore, that in the 0·3298 gramme of 'Scandia,' on which Nilson worked, there must have been only 0·043 gramme of the new oxide and seven or eight times as much ytterbia. Clève chooses 45 as representing most closely the atomic weight of scandium. Scandia, $Sc_2 O_3$, is a perfectly white, light powder, infusible, and resembling magnesia. Acids, even the strongest, attack the oxide only with difficulty; it is, however, more readily soluble than alumina. Its density is 3·8. The hydrate, like that of alumina, is white and bulky. It does not absorb carbonic acid from the air, is insoluble in an excess of ammonia or potash hydrate, and does not decompose ammonium chloride. Its salts are colourless, with an acrid, astringent taste, quite different from the other salts of the metals of the yttrium group. The sulphate does not crystallize distinctly; the nitrate, oxalate, and acetate and formiate, on the other hand, form crystals. The chloride gives no spectrum when heated in a gas flame. Its solution is precipitated by ammoniac and potassic hydrate, and the precipitate is not soluble in an excess of either reagent. Tartaric acid prevents the precipitation by ammoniac hydrate in the cold. Sodium carbonate gives a precipitate soluble in excess of the reagent. Sulphuretted hydrogen gives no precipitate; ammonium sulphide throws down the hydrate; sodium phosphate gives a gelatinous precipitate; oxalic acid gives a curdy precipitate which soon becomes crystalline. Sodium hyposulphite and sodium acetate at once cause precipitates in boiling solutions; the precipitation, however, is incomplete. The discovery of scandium is of peculiar interest, from the fact that its existence and properties were predicted by Mendelejeff, as a consequence of his law of periodicity, and called by him *Ekabor*. The remarkably close correspondence between the properties of ekabor and those of scandium is shown in Clève's paper, by printing them in parallel columns. (*Compt. Rend.*, 1879, lxxxix. 419.)

On Two New Elements, Thulium and Holmium.—Since the ytterbium of Marignac and the scandium of Nilson, both of which were discovered in erbia, give colourless salts, Clève endeavours to distinguish the substance in this earth which gives the red colour and the beautiful absorption spectrum

to its salts, in order to ascertain if it was erbium itself. Employing the residues from which Nilson had separated ytterbia and scandia, he found it impossible to obtain a red oxide with a constant molecular weight, even after several hundred decompositions. Suspecting the presence of the unknown oxide he induced Thalèn to examine the spectrum of what he regarded as the purest erbia, and to compare it with that of yttria and ytterbia. Certain absorption bands in the last fractions suggested that the colour of erbia is due to the presence of three oxides giving absorption spectra. The reddest of the fractions (RO molecular weight = 125–127) were united and submitted to a long series of decompositions, one fraction being treated for ytterbia, another for yttria, and a third, intermediate, containing the concentrated erbia. At the same time, he attempted to concentrate the colouring matter in residues A, rich in ytterbia, and B in yttria. After pushing the treatment as far as possible with the amount of material at his disposal, he handed over the five fractions to Thalèn, who found bands common to all the fractions, and due probably to erbia. The following bands varied markedly from one fraction to another:—

	Wave length.	*Fraction A.* Extr. from ytterbia residue.	Extr. from Erbia, 126–127.	*Erbium?* Mean fractions, 126–127.	*Fraction B.* Extr. from Erbia, 126–127.	Extr. from residues rich in yttrium.
x	6480	strong	quite strong	fails	fails	fails
y	6400–6425	fails or trace	trace	weak	weak	pretty stron
z	5360	fails	fails or trace	trace	feeble	quite strong

Hence x belongs to fractions situated near ytterbia, and does not exist in fractions from yttrium. But, on the other hand, y and z fail in the ytterbium residues, but grow more distinct as yttrium is approached. The ytterbia fractions gave a rose-colour with a tinge of violet; the yttria fractions had an orange tint. For the element coming between ytterbia and erbia, characterized by band x in the red of the spectrum, Clève proposes the name of *Thulium*, from Thule, the ancient name of Scandinavia. The atomic weight, Tm, should be about 113 if its oxide be RO. Erbium proper, which has the common bands mentioned, has the atomic weight 110–111. Its oxide is of a light rose colour. The third metal, characterized by the y and z bands, and which is between erbia and terbia, should have an atomic weight below 108. Its oxide appears to be yellow. For it the name of *Holmium* is proposed, derived from the Latin name of Stockholm, the environs of which are rich in yttria minerals.—(*Compt. Rend.* 1879, lxxxix. 478.)

Notes on the two new Elements announced by Clève.—Soret points out that he showed in the spring of 1878 that the two bands which characterize holmium do not belong to erbia, but to a new earth which he called provisionally X, and which is perhaps identical with philippium, since discovered by Delafontaine. Besides these two bands, Soret recognized three other absorption bands; one less refrangible than A, a second overlapping the band of erbia in the indigo, and a third (faint) in the violet a little beyond *h*. In the ultra violet spectrum six absorption maxima exist between H and R. In samarskite the earth X is relatively to erbia much more abundant than in gadolinite. As to the red ray which characterizes thulium, Soret had already observed that also in some ytterbia products which had been

sent to him for examination by Marignac. Lecoq de Boisbaudran confirms Soret's statement in regard to the red-thulium ray, having observed it in a sample of impure yttria which he had received from the latter some months ago. He inclined to the belief that all the bands were caused by erbia, modified by the conditions. But special experiments led him to agree with Soret, and to accept the possibility that erbia was a mixture of three oxides.—(*Compt. Rend.* 1879, lxxxix. September.)

Oxidation of Formic and Oxalic Acid by Ammonium-copper oxide.—The oxidating action of this substance on azotized bodies has already been pointed out by Löw. Cazeneave now finds that it has a similar action on formic and oxalic acid. At ordinary temperatures and at 100° no change occurs, if the substances be heated together in closed tubes; at 150°, however, both acids are rapidly oxidized to carbonic acid:—

$$CH_2O_2 + 2\,CuH_2O_2 + 2\,NH_3 = CO_3H_2(NH_3)_2 + Cu_2O + 2\,H_2O.$$
$$C_2H_2O_4 + 2\,CuH_2O_2 + 4\,NH_3 = 2\,CO_3H_2(NH_3)_2 + Cu_2O + H_2O.$$

If the solutions be subsequently exposed to the air, oxygen is taken up and ammonium-copper carbonate is formed.

$$CO_3H_2(NH_3)_2 + Cu_2O + O = CO_3Cu(NH_3)_2 + CuH_2O_2.$$

When the formic and oxalic acid are in large excess, the copper oxide is reduced to copper.—(*Bull. Par.* 1879, xxxii. 277.)

The Presence of Alcohol in Animal Tissues.—To clear up some points hanging on the investigations of Schrader and Dusch, the following experiments were tried by J. Bechamp (*Compt. Rend.* 1879, lxxxix, 573). A piece of horseflesh, weighing three kilog., was dipped in boiling water for ten minutes, and placed in a dish on the 8th June, and closely covered up with a thick linen cloth. On the 16th July the flesh had become very foul and full of life, but the air does not appear to have penetrated to the centre. Alcohol amounting to 0·8 gramme was obtained, a part of which was burnt, the rest oxidized with chromic acid to aldehyde, and then to acetic acid, the soda salt of which was prepared and identified. In addition to this, about ten grammes of sodium acetate, butyrate, and salts of other higher acids, were obtained. Another mass of horseflesh, weighing four kilog., was simply left to itself for four days, and on subsequent treatment yielded alcohol as before, but in less quantity, as well as acetic and butyric acid. The next question which suggested itself to the author was to determine whether alcohol forms a constituent of a living organ. It has been shown by A. Béchamp that alcohol is a normal constituent of urine and milk; the question hence arises whether it also occurs in the tissues. Fresh sheep's liver, immediately after the animal was killed, contained alcohol; fresh and still warm sheep's brains also contained it, and in larger quantity than the liver; fresh and still warm bullock's brains also contained it. These results show that the presence of alcohol in the tissues does not necessarily indicate poisoning.

GEOLOGY AND PALÆONTOLOGY.

Formation of Mountains.—M. de Lapparent has an excellent article on this subject in the *Revue des Questions Scientifiques* for July last. He points out that the changes of position, the elevation, and especially the folding, of strata observed in mountainous districts are due to energetic lateral compression. Elie de Beaumont taught that mountain-chains do not occupy the centres of continents and show symmetrical slopes on both sides, but that they are to be found near the sea, and have a precipitous slope on the side facing the sea, whilst the opposite side slopes gently away, forming the mass of the continent, and usually terminates in the opposite ocean by a line of low country. This view has been formulated as a law by several geologists, especially in America, where the long line of the Rocky Mountains and the Andes furnish such a striking example in its support; and Professor Dana has added the following rider to it, that when two chains of elevations form the two shores of a continent, the one facing the largest ocean is the higher one. M. de Lapparent indicates that in order to apply these principles to the Old World mountains, and especially to those of Europe, it is necessary to force the facts a little, and hence he is led to the belief that while it is perfectly true that chains of mountains are always formed in the vicinity of the ocean, it is necessary, in order to understand their distribution, to consider the geographical conditions that prevailed at the period of their formation. He sums up his views in the following formula:—'At the epoch when a chain of elevations has just attained its principal relief, it consists of two slopes of very unequal inclination, one of which, gently inclined, is connected with the continent, while the other, which is abrupt, directly faces the sea.' Thus the Pyrenees, which are shown by geological evidence to have been elevated after the formation of the Nummulitic and before that of the Miocene deposits, were united by a gradual slope towards the south with the Spanish continent, while the foot of the precipitous northern face was washed by the Miocene sea. The Alps date from the interval between Miocene and Pliocene times. To the north they joined on by a gentle slope to the plains of northern Germany, while towards Lombardy they formed a vertical wall, at the foot of which were deposited the sediments of the Sub-apennine sea. M. de Lapparent refers to other European chains, and then formulates the following general law:—'A chain of mountains, at the moment of its formation, always occupies a littoral situation; it does not depart from this afterwards, except when the continent is enlarged by new additions obeying the same law. If, therefore, at the present day, the Scandinavian mountains on the one hand and the Cordillera of the Andes on the other, emerge directly from the depths of the ocean, this is because these two chains belong to the most recent formations which have been produced on the globe; and geology, as is well known, fully justifies this conclusion.'

From the consideration of the soundings which have been so rapidly accumulated of late years, M. de Lapparent arrives at the conclusion that the great depths of the sea, as a general rule, are the counterparts of the great elevations of the land, and lie directly at their base, and hence he concludes that 'at the moment when the profile of one of the lines of relief of the

earth's surface becomes strongly marked, this profile includes an abrupt central line, joining at its extremities with two gently-inclined lines; of the two angles thus formed that which has its apex outward constitutes the crest of a mountain-relief, while the other forms the ridge of a marine depression. In this way, at the moment when a great shore-line is constituted, it is marked on the one hand by a projecting chain, the origin of a continent, and on the other by a deep fosse, in which the sea collects; the projecting chain moreover may only emerge in part.' These correlated elevations and depressions are considered by M. de Lapparent to be due to foldings in the comparatively thin crust of the earth caused by the contraction of its fluid nucleus. M. de Lapparent's article contains many remarks of interest to geologists upon various matters more or less connected with, or explained by his view of the origin of mountains, and it will well repay careful perusal. He summarizes his results as follows:—'All the inequalities of the surface of the globe have a single cause, which is incessantly in action, although it must probably manifest itself only at intervals, namely, the contraction of the fluid nucleus in losing its heat, whence proceeds, for the solid envelope, the necessity of adapting itself continually to the new form imposed upon it by the conditions of its equilibrium. It is thus that, since the earliest ages of the globe, the continents have been formed by successive additions, which gradually rendered their contour more and more complicated. The surface of the sea has constantly diminished in extent, but at the same time its depth has constantly increased with the elevation of the continents. Hence have arisen those diverse physical conditions, in which the natural effects of latitude are complicated by a thousand modifications due to the nature of the soil, to altitude, to exposure, to vicinity to, or distance from, the sea. Thus all those external conditions, the variety of which gives so great a charm to our globe, at the same time that it is the most powerful of stimulants to human activity, are contained in their germ in the law that we have laid down.'

Chalk Flints.—At the last meeting of the Geological Society (December 19[illegible]), Dr. Wallich read a contribution towards the solution of that *quæstio vexata*, the origin and mode of formation of the flints which occur so abundantly in certain parts of the Cretaceous series. Taking as the basis of his conclusions the fact brought to notice by him in 1860, namely, that the whole of the Protozoan life at the sea-bed is strictly limited to the immediate surface-layer of the muddy deposits, he pointed out in detail the successive stages of the flint-formation, from the period when the chief portion of the silica of which they are composed was eliminated from the ocean-water by the deep-sea sponges, to the period when it became consolidated in layers or sheets conforming to the stratification of the Chalk. In relation to this subject the author claims to have sustained the following conclusions:—1. That the silica of the flints is derived mainly from the sponge-beds and sponge-fields, which exist in immense profusion over the areas occupied by the Globigerine or calcareous 'ooze.' 2. That the deep-sea sponges, with their environment of protoplasmic matter, constitute by far the most important and essential factors in the production and stratification of the flints. 3. That whereas nearly the whole of the carbonate of lime, derived partly from Foraminifera and other organisms that have lived and died at the

bottom, and partly from such as have subsided to the bottom only after death, goes to build up the calcareous stratum; nearly the whole of the silica, whether derived from the deep-sea sponges or from surface Protozoa, goes to form the flints. 4. That the sponges are the only really important contributors to the flint-formation that live and die at the sea-bed. 5. That the flints are just as much an organic product as the Chalk itself. 6. That the stratification of the flint is the immediate result of all sessile Protozoan life being confined to the superficial layer of the muddy deposits. 7. That the substance which received the name of '*Bathybius*,' and was declared to be an independent living Moneron, is, in reality, sponge-protoplasm. 8. That no valid *lithological* distinction exists between the Chalk and the calcareous mud of the Atlantic; and *pro tanto*, therefore, the calcareous mud may be, and in all probability is, 'a continuation of *the* Chalk-formation.'

Bohemian Permian Amphibia.—A well-known Bohemian palæontologist, Dr. Anton Fritsch, has commenced the publication of a monograph on the fauna of the coal and limestone of Permian age, occurring in Bohemia. A most interesting assemblage of fossils has been discovered in these rocks within the last ten years. It includes, at present, about forty-three species of amphibians, thirty-three fishes, eleven arthropods, and an *Anthracosia*, and many of the forms are exceedingly remarkable. They are being described in detail and admirably figured by Dr. Fritsch, in the work *Fauna der Gaskohle und der Kalksteine der Permformation Böhmens*, the first part of which has quite recently appeared. In this the author gives an account of the geological formation from which the remains described have been obtained, and other general information connected with the fossils, and then describes the species of Amphibia, preceding his descriptions with some generalities upon the group to which they belong. He refers all the forms noticed in his first part to Cope's Order Stegocephali, which comprises the Labyrinthodontia, Ganocephala, and Microsauria of other writers, and places them under four genera, namely, *Branchiosaurus*, *Sparodus*, *Hylonomus*, and *Dawsonia*.

Branchiosaurus resembles the Earth-salamanders in possessing gills, and the author remarks that in their broad rounded head, short thick body, well developed limbs, terminating in digits, and their rudder-like tails, they suggest comparison with larval forms of the existing Urodela. *Branchiosaurus salamandroides* attained a length of two and a half inches; its skeleton was ossified early in life, the bones being completely defined in specimens only a quarter of this length. The skin was dense and its impression is preserved in most of the specimens; when highly magnified, ridges are seen upon it, constituting the first indications of scales, which become more developed on the lower surface of the body. The general form of the head is nearly semicircular; the maxillaries and premaxillaries carry numerous short pointed teeth; the orbits are rather large, and in well-preserved examples show a circle of about fourteen sclerotic bones; the lower jaw is attenuated in front, composed of three elements,—articular, angular, and dentary, the latter having about twenty teeth; and on each side of the hinder part of the skull there are two branchial arches, which bear two rows of small, spheroidal bones, each furnished with a curved spine. The trunk vertebræ are about twenty in number and all except the first bear ribs. The sacrum consists of a single

vertebra, and beyond this come about twenty-one caudals, also bearing ribs. Four other species of *Branchiosaurus* are described.

Sparodus, of which two species (*S. validus* and *crassidens*) are noticed, is nearly allied to the genera *Hylerpeton* and *Batrachiderpeton*. The skull is more wedge-shaped than in *Branchiosaurus*: the vomer and the palatine bones bear numerous teeth. The genus *Hylonomus* is recognized from small portions of jaws, believed to indicate two species; and allied to this genus is a new one, which the author names *Dawsonia*, after the distinguished Canadian geologist and palæontologist who established the former genus.—*Geological Magazine*, November, 1879.

Concretions and Inclusions in Granite.—Mr. J. A. Phillips has communicated to the Geological Society (November 19, 1879) the results of his investigations of the discordant patches often met with in granites. These patches are sometimes angular, sometimes rounded, sometimes clearly defined, at others gradually melting away into the surrounding mass, but generally finer in texture than the latter. The chemical and microscopic examination of such patches in the granites of Cornwall, Shap Fell, Aberdeen, Peterhead, Fort William, and North-eastern Ireland, has led Mr. Phillips to distinguish two classes of these inclusions, namely:—1. Concretionary patches, the result of the abnormal aggregation of the minerals constituting the granite itself, generally containing more plagioclastic felspar, mica, or hornblende than the latter, and in all probability formed contemporaneously with the solidification of the mass; and 2. Included fragments of schistose or slaty rocks, often not very highly altered, caught up from the rock-masses through which the granite has forced its way. In the discussion which ensued upon the reading of Mr. Phillips' paper, Mr. G. W. Ormerod stated that the concretionary patches were to be seen on the eastern side of Dartmoor, where they are generally of an oval form and are called 'mares' eggs;' and Professor Ramsay remarked that fine examples of them are visible on the steps of the Duke of York's Column.

British Secondary Comatulæ.—Mr. P. Herbert Carpenter has described before the Geological Society (3rd December, 1879) seven new Comatulidæ from the Cretaceous and Oolitic series of Southern England, and given some new facts respecting *Glenotremites paradoxus* of Goldfuss from the Upper Chalk. This species is remarkable for the presence of certain characters which are very conspicuous in the recent *Antedon Eschrichtii*, and also in a new species dredged by the *Challenger* at Heard Island, in the South Atlantic, namely, the presence of strong ribs on the inner wall of the centrodorsal, five of which, interradial in position, are much more prominent than the rest. So far as is yet known, these features occur in no other recent Comatula, with the exception of one species from the South Pacific, in which there is a faint indication of such ribs; but they are all equal. Another Antedon species is described from the Chalk of Sussex. It differs from *Antedon paradoxus* in the absence of these ribs, and in the shallowness of the centrodorsal cavity.

Two species were described from the Gault of Folkestone. One is an Antedon, with no special relations to any recent forms. It might have lived as well at 20 as at 500 fathoms. But the other species is an Actinometra, possessing certain characters only known to occur in species from quite shallow water, 20 fathoms or less, in the Philippine Islands and Malay Archi-

pelago. The centrodorsal is a flat plate, nearly on a level with the surface of the radials, or sometimes even below them, separated from them by clefts at its sides, and entirely devoid, not only of cirrhi, but also of cirrhus-sockets. This condition is only an extreme stage of the metamorphosis of the centrodorsal piece, which bears cirrhi for a time after its liberation from the larval stem; but these cirrhi eventually disappear and their sockets become obliterated. The *Challenger* collection contains a series of specimens of *Act. Jukesii*, from Torres Straits, which illustrate this point very completely; and it is, therefore, of no small interest to find a fossil Comatula which shows one of the extreme stages of the metamorphosis.

The large size of the three Antedon species from the Chalk and Gault is very remarkable. *Ant. paradoxus* has a centrodorsal half as wide again as that of any recent form; while *Ant. Eschrichtii* is the only recent species with a centrodorsal approaching the size of those of the other Chalk Antedon, and of that from the Gault. *Act. Lovéni*, from the Gault, however, and the older Comatulæ, all had small calices like most recent species. An elegant centrodorsal (*Ant. rotundus*) is described from the Haldon Greensand, and also two species from the Bradford Clay. One is an Antedon, the oldest known, with no special characters; the other is an Actinometra, with a a centrodorsal essentially like those of species now living in shallow water in the Philippines and Malay Archipelago. The oldest known Comatula, an Actinometra from the Bath Oolite, has similar relations.

Argillornis longipennis.—Some two years ago Professor Owen described before the Geological Society the humerus of a large bird from the London Clay of Sheppey, for which he proposed the above name (see *Popular Science Review*, New Series, Vol. ii., p. 106). He was inclined from the first to regard this bone as belonging to the wing of a large aquatic bird; and the recent discovery of a portion of a bird's skull in the same deposit would seem to be confirmatory of Professor Owen's views. In this specimen, which was described by the Professor before the Geological Society, on the 5th November, the lower jaw and the fore-part of the upper jaw are deficient; and the characters presented by it, like those of the humerus previously described, were said to approximate the fossil most nearly to the Albatross among existing birds, although, like *Odontopteryx*, it differed from *Diomedea*, and also from the Cormorant and the Totipalmates generally, in the absence of the basirostral external nares and of the supraorbital gland-pits. The present fossil differs from *Odontopteryx* in having the fore-part of the frontal broader and the upper tract of the bill less defined, as also in some other characters; but no comparison of the palatal structure can be made upon the existing specimens. In point of size, taking the Albatross as a term of comparison, this skull may well have belonged to a bird with wings of the extent indicated by the humerus already described; and the resemblance of the skull to that of the Albatross would also seem to be confirmatory of the specific collocation of the two specimens. The presence of four small pits or perforations on the only part of the alveolar border which appears to be uninjured, led the author to conjecture that the bird may have been dentigerous; but this seems to be very uncertain.

MINERALOGY.

Amblygonite.—The new mineral species, triploidite, described by Brush and Dana, is shown by them to be isomorphous with wagnerite, and closely related in composition to triplite. These three minerals have respectively the formulæ $(Mn, Fe)\ P_2O_8 + (Mn, Fe)\ (O\ H)_2$, $Mg_2\ P_2O_8 + Mg\ F_2$ and $(Fe, Mn)_2\ P_2\ O_8 + (Fe, Mn)\ F_2$. From a comparison of these formulæ it is argued that the relation between the minerals requires the assumption that the hydroxyl in triploidite must play the same part as the fluorine in the other two. In a paper on the chemical composition of amblygonite, by Samuel L. Penfield (*Amer. Jour. Sc.*, 1879, xviii. 295), the writer endeavours to show that in this mineral also the hydroxyl group is isomorphous with fluorine, and that in chemical composition the original amblygonite does not differ from the American and Montebras varieties, which have been called hebronite. He also shows from the results of his analysis, that a new and more simple formula than any previously accepted must be taken to represent its composition. The specimens examined are three from the Maine localities, from Branchville, Connecticut, two varieties from Montebras, and one from Penig, Saxony. All the specimens gave numbers approaching closely to the ratio 1 : 1 : 1 : 1 ; hence he proposes the formula $Al_2\ P_2\ O_8 + 2\ R\ (O\ H, F)$, or $\left.\begin{matrix} 3\ Al_2\ P_2\ O_8 \\ 2\ R_2\ \ P\ O_4 \end{matrix}\right\} + \left\{\begin{matrix} Ae_2\ (O\ H, F) \\ 2\ R\ (O\ H, F) \end{matrix}\right.$ 6, as the true formula for all varieties of the mineral. Des Cloizeaux, from a difference of optical characters made out by him, has divided the mineral into two species: the original amblygonite, including the specimens for Penig, Saxony, and from Montebras, France; and a second species, for which he proposes the name of montebrasite, the hebronite of Von Kobell, including all the other localities. Owing to the close identity in chemical composition, it seems that a slight variation in optical properties is hardly sufficient ground for dividing the mineral into two species; and it is proposed that the old name of amblygonite should be retained, and that the varieties be included in it.

PHYSICS.

Photometric Researches on Coloured Flames form the subject of an important communication by M. Gouy to the *Annales de Chimie et de Physique*. In an introduction he gives a summary of what has been done in this department since the work of Bouguer, in 1729. His photometer—those of Romford, of Foucault, and of Bunsen, the double refraction instrument of Arago, that by polarised light of Becquerel—are adverted to, all of which are exclusively confined to white light. The first prismatic photometer seems due to M. Gouy, and a very similar instrument to Vierordt. The latter consists of a spectroscope with two continuous slits, the breadth of which can be varied by means of a micrometer screw. The spectra being adjusted to equal intensity, the screw gives the relative breadth of the slits, and, consequently, the comparative brightness of the sources of light. Monsieur Gouy has successfully employed two photometers. The former acts by throwing on the slit

of a spectroscope the image of Babinet's compensator, arranged to give horizontal lines, and causing it to be traversed by two luminous pencils, rectangularly polarized, producing a spectrum marked with horizontal lines. Each pencil gives a separate system of lines, and the dark portions of one being superposed on the bright parts of the other, it is always possible, by varying the intensity of one light by a known quantity to extinguish the lines in one portion of the spectrum. The apparatus is arranged as follows: on the axis of the collimator are placed the following pieces,—An achromatic lens; a Nicol's prism, with its principal axis horizontal; a Babinet's compensator, of peculiar form; an achromatized prism of Iceland spar; a Nicol movable in graduated circle, and a lens. Between the last Nicol and the spar prism, the tube supports a lateral branch at a right angle, containing a total reflection prism and a lens. A flame is placed in front of this side branch, its rays pass through the prism as extraordinary, whereas the opposite occurs with flames placed in front of the movable Nicol. They thus give complementary lines, either series of which can be varied in intensity at will. This instrument had the defect of requiring great intensity of light: and it has been materially improved.

The more recent form is that of a two-prism spectroscope, the second prism of which is movable round the centre of the table, and is attached to the arm bearing the telescope. Before the object-glass of the collimator is a flat mirror covering its upper half. On this are reflected the rays coming from a second collimator, so that if before the two collimator slits are placed two identical sources of homogeneous light, there will be seen the two images coincident in the focus of the observing telescope. There will, therefore, be two semicircles of unequal brilliancy. To equalize them the second collimator has two Nicol's prisms in its axis, one fixed, the other movable: a slit is substituted for the eye-piece of the observing telescope. The coloured flame to be studied is placed before the first-named collimator, and the standard flame before the second.

A method of producing constant coloured flames by means of pulverized saline solutions, forming part of the necessary apparatus, is described at length. Observations follow on sodium flames; on the transparency of coloured flames, for their proper and for heterogeneous radiations; on the density of metallic vapours, and on the reducing or oxydizing portions of solid conical masses of heated gas.

Optical power of Spectroscopes.—Lord Rayleigh notes in the *Philosophical Magazine* that as the power of a telescope is measured by the closeness of the double stars which it can resolve, so the power of a spectroscope ought to be measured by the closeness of the closest double lines in the spectrum which it is competent to resolve. In this sense it is possible for one instrument to be more powerful than a second in one part of the spectrum, while in another part the second instrument is more powerful than the first. The most striking cases of this inversion occur when one instrument is a diffraction- and the other a dispersion-spectroscope. If the instruments are of equal power in the yellow region, the former will be more powerful in the red and the latter in the green. That the resolving power of a prismatic spectroscope of given dispersive material is proportional to the total thickness used, without regard to the number, angles, or setting of the

prisms, is a most important—perhaps the most important—proposition in connexion with this subject. Hitherto, in descriptions of spectroscopes, far too much stress has been laid upon the amount of dispersion produced by the prisms. But this element by itself tells nothing as to the power of an instrument. It is well known that by a sufficiently near approach to a grazing emergence the dispersion of a prism of given thickness may be increased without limit, but there is no corresponding gain in resolving-power. So far as resolving-power is concerned, it is a matter of indifference whether dispersion be effected by the prisms or by the telescope. Two things only are necessary: first, to use a sufficient thickness; secondly, to narrow the beam until it can be received by the pupil of the eye—or rather (since with full aperture the eye is not a perfect instrument), until its width is not more than one third or one fourth of the diameter of the pupil.

Dispersion of Dark Heat-rays, and Measurement of their Wave-lengths, was, according to M. Mouton, first attempted by Herschel, in 1800; it has since been the subject of many researches, which may be divided into two groups. The former, excluding all hypotheses as to the nature of the radiation, endeavours to determine either the position of the heat-maximum, and its dependence on the material and thickness of the prism, or the laws of absorption and transmission proper to the different parts of the spectrum. The latter are so much more important as to need a chronological sketch. In 1818 Berard showed that solar heat is polarized by reflection, and undergoes double refraction. In 1834 Forbes discovered the action of tourmaline and of bundles of mica: Melloni and Biot, the rotation by quartz of the plane of polarization; Wartmann, that by magnetic force. In 1847, Fizeau and Foucault produced interference-phenomena by diffraction; and then added the coincidence in the light spectrum, and the continuation in the ultra-red of the bright and dark bands, obtained either by a crystal plate or by a perpendicular lamina of quartz between two polarizers. The same year there appeared the researches of Knoblauch, Provostaye, and Desains, who established—(1.) That the two heat-pencils emerging from Iceland Spar are completely polarized: one in the principal axis, the other at right angles to it. (2.) That polarized heat follows the laws of light. (3.) That the variations of intensity, after reflection from glass at different incidences, agree with Fresnel's formula. (4.) That there is perfect resemblance between the phenomena of polarized light and heat when reflected from polished metals. They also measured accurately the rotation of the plane of heat-polarization produced by a magnet. In 1850 they studied the polarizing action of glass bundles, and determined the rotatory power of turpentine and sugar solutions for heat. M. Desains, by means of bundles of rock-salt plates, was able to polarize in definite quantity the rays of heat emanating from a source at about 300° Cent., which fail to pass through glass, finding them to be conformable to Fresnel's laws for glass and light. M. Mouton continues what he calls the 'graduation of the heat-spectroscope,' using a modification of M. Fizeau's method: a plate of quartz, cut parallel to the axis, being placed between two parallel Nicol's prisms. The source of heat was a Bourbouze lamp, with a hood of platinum, heated to a white heat by means of gas and compressed air. This was placed in a neighbouring room, communicating with the other apparatus by means of a lens forming an inverted image at

fifty centim. where the slit was situated, the polarizing instruments being between the lens and the slit, and consisting of a double refracting prism of spar with a central extraordinary image for analyser, and a large Nicol for polarizer. The quartz plate was between these. The prisms of quartz were on the coupled principle already employed by MM. Gouy and Thollon (see above). Achromatic lenses were used for collimator and telescope, a linear thermopile being in the principal focus of the latter. Tables of results thus obtained lead to a fine curve, in which abscissæ are wave-lengths, and ordinates, values of $(n'-n)$, or the difference of the indices, which are functions of the wave-lengths and of the substance of the plate.

Harmonic Ratios in the Spectra of Gases have been noted by Professor Johnstone Stoney for three of the hydrogen lines, and others by Soret and Lecoq de Boisbaudran. Mr. Arthur Schaster draws attention in *Nature* to a series which he has found in the Iron-spectrum, of which, however, he has only as yet examined a seventh part. He states them in a tabular form, giving in a first column the corrected wave-length, according to Angström; in a second, a ratio expressed fractionally; in a third, the effect of multiplying the first column by the second; and in a fourth, the nearest observed value of other lines: the last column shows the difference between computed and observed values.

A second table gives a set of iron-lines which can be arranged as Harmonics of a fundamental wave-length of given magnitude. The differences are all small, never reaching an integer, and in one instance sinking to zero. He cannot yet say definitely whether all these coincidences are due to accident. The true law of distribution, however, he thinks not to have been yet found, though harmonic ratios may take a secondary part in it.

A Fresh Experimental Determination of the Velocity of Light has been undertaken by Mr. Nicholson, of the U.S. Navy. He uses, in the main, the same apparatus as was employed by Foucault, namely, a revolving mirror and reflected image. But in Foucault's experiments the deflection was too small to be measured with the required accuracy. It amounted to only a fraction of a millimetre, whereas in these it exceeded 133 millimetres, being about two hundred times that obtained by Foucault. It could be easily increased. The distance between the mirrors was nearly 2000 feet, the radius or distance of the revolving mirror from the slit was about 30 feet, and the speed of the mirror about 256 revolutions per second. The deflection was measured within three or four hundredths of a millimetre in each observation, so that the result is probably correct in this respect to within a ten thousandth. A shed was erected at one end of the line, in one corner of which a heliostat reflected the sun's rays through the slit to the revolving mirror, and thence to the distant mirror. The shed was blackened inside. The revolving mirror consisted of a cast-iron frame, containing, between hardened steel points, an arbor carrying the mirror—a disc of plane glass about 1¼ in. diam., silvered on the hither side. A small turbine in a box on the same axis was rotated by the entrance of compressed air striking its vanes, and by the reaction of the same air in escaping. A disc above the mirror served to bring the centre of gravity into the axis of rotation, by means of a series of position-testings. It gave no sound when properly balanced, and needed only occasional lubrication.

The apparatus for measuring deflection consisted of a slide-rest, carrying an adjustible slit, and an achromatic eye-piece, with a single silk fibre in its focus. A piece of plane glass at an angle of 45° is next the eye. The eye-piece was moved till the fibre bisected the deflected image of the slit.

To regulate and measure the speed of rotation, a tuning-fork, bearing on one prong a steel mirror, was employed. This was kept in vibration by means of electricity. It was so placed that the light from the revolving mirror was reflected to the piece of plane glass in the eye-piece and thence into the eye. When fork and revolving mirror are both at rest, the eye sees an image of the revolving mirror; when the fork vibrates this image is drawn out into a band of light. When the mirror begins to revolve this band breaks up into a number of moving images of the mirror; and when finally the mirror makes as many turns as the fork makes vibrations, these images are reduced to one which is stationary. Hence to make the mirror execute a given number of turns, it is only necessary to open the valve, until the images of the revolving mirror come to rest. The electric fork was compared with a standard *Ut* 3 fork, the temperature being noted. The beats were counted for 60″.

The lens was 8 in. in diameter, not achromatic; focus, 150 ft. The stationary mirror was 7 in. in diameter, silvered on the front surface. The distances between the various parts of the apparatus having been determined, the heliostat and distant mirror were adjusted, and the electric fork connected against the standard for 60″. The revolving mirror was then started, and regulated until the image came to rest near the cross-hair. The screw was then turned until the cross-hair bisected the deflected image of the slit. This was repeated until ten observations were taken, and then the temperature was again taken, and the beats noted. Usually five sets of observations were taken morning and evening.

The distance between the two mirrors was taken on an average of five measurements, and found to be 1986·23. The rate of vibration of the standard fork was found by allowing it to trace its record on the lamp-blacked cylinder of a chronoscope, time being given by a sidereal break-circuit chronometer and a Ruhmkorff coil.

The direction of rotation was at first right-handed, and afterwards reversed.

The effects of the vortex of air about the mirror could be found at any speed. To prevent bias in observation, readings were taken by different observers independently of each other.

The result obtained for the velocity of light, *in vacuo*, was 299·828 kilometres per second. Foucault's determination in C.G.S. units being $2·98 \times 10_{10}$ or 298 kilometres per second.

ZOOLOGY.

Development of Oysters.—According to the generally received opinion, the eggs of the oyster are fertilized inside the shell of the parent, within the mantle-cavity of which the young are carried until they are provided with

shells of their own; they leave the parent at a tolerably advanced stage of development, and the period of their free existence is very short. Mr. W. K. Brooks states that in the American oysters things go on very differently. Finding that no young oysters were to be met with in the mantle-cavity of the parents, he tried artificial fertilization of ova taken from the ovaries, and was completely successful,—raising millions of young oysters, and tracing them through all their stages of development up to the time when they had acquired all the characteristics which Salensky, Lacaze-Duthiers, Möbius, and others, have figured and described in the young European oyster at the time it leaves its parent. On the other hand, he never found young oysters inside the mantle-cavity of an adult, although from the state of the ovaries the individuals examined were evidently engaged in spawning. Mr. Brooks gives the following statement of the general results of his investigation:—

1. The oyster is practically unisexual; at the breeding season each individual contains either eggs or spermatozoa exclusively.

2. Segmentation of the egg takes place very rapidly.

3. Segmentation is completed in about two hours and gives rise to a gastrula, with ectoderm, endoderm, digestive cavity, and blastopore, and a circlet of cilia or velum. At this stage of development, the embryos crowd to the surface of the water and form a dense layer, less than a quarter of an inch thick.

4. The blastopore closes; the endoderm separates entirely from the ectoderm, and the two valves of the shell are formed, separate from each other, at the edges of the furrow formed by the closure of the blastopore.

5. The digestive cavity enlarges and becomes ciliated, and the mouth pushes in as an invagination of the ectoderm at a point directly opposite that which the blastopore had occupied. The anus makes its appearance close to the mouth.

6. The embryos scatter to various depths, and swim by the action of the cilia of the velum. The shells grow down over the digestive tract and velum, and the embryo assumes a form so similar to various marine lamellibranch embryos which are captured by the dip-net at the surface of the ocean, that it is not possible to identify them as oysters without tracing them from the egg. The oldest ones that he succeeded in rearing in aquaria were exactly like the embryos of *Cardium*, as figured by Lovén.

7. The ovaries of oysters less than one and a half inch in length, and probably not more than one year old, fertilized with seminal fluid from males of the same size, developed normally.

An illustrated report on these highly important and interesting investigations will appear shortly in the Report of the Maryland Fish Commission for 1879.—*Silliman's Journal*, December, 1879.

CHAMÆLEONS.

By Professor J. REAY GREENE, B.A., M.D., F.L.S., F.Z.S., &c.

[Plate III.]

IN the system of nature Chamæleons unquestionably occupy a more conspicuous place than any other family of reptiles now living upon our globe. This family constitutes one of the three sub-orders under which most herpetologists, following Stannius, arrange existing Lizards. On no family of Crocodilians, Tortoises or Snakes can a like dignity be imposed * The structure of the Chamæleons is, in many respects, very remarkable; their habits, especially those of the common species, are yet more striking. Not even the sloths are so entirely adapted to lead a purely arboreal life. Slower in movement than the tortoises, the common Chamæleon is nevertheless gifted with apparatus for the pursuit and capture of winged prey, which it finally seizes with the most unerring rapidity. Throughout the whole animal kingdom no more notable adaptation of means to end can be said to exist. Such means are at once active and passive; they are numerous and diverse. The long extensile curiously-constructed tongue, the exceptionally mobile eyes with their manifold adjustments, the changing skin, the slender limbs, specially modified for climbing, and the prehensile tail,

* On such questions, as to the systematic value of certain groups of reptiles, there is more or less divergence of opinion. Thus Agassiz would have separated the turtles, as a sub-order, from the remaining tortoises, whilst some have suggested a like separation of the family of geckos from other lizards. But these views have not been generally accepted. Again, the typhlopine serpents differ much in the structure of their skull not only from other serpents but from reptiles in general. Yet in the judgment of Johann Müller, who is here followed by most modern anatomists, these remarkable snakes do not constitute a group of higher rank than that of a family (Typhlopidæ). Among tortoises and batrachians, also, very notable differences as to cranial structure may exist without being accompanied by corresponding diversities, at all comparable in extent or apparent importance, in other parts of their organization. Extinct reptiles, such as Ichthyosaurus, are excluded from this comparison.

terminating a carcase unparalleled for meagreness, are not the only parts of the Chamæleon which demand attention.

It is true that a rank equal to that usually conferred on the Chamæleons might be awarded the singular Hatteria (*Sphenodon* or *Rhynchocephalus*) of New Zealand, whose characters have been so well described by Dr. Günther.* It, too, recedes from the typical lizards, while it approaches the crocodilians and other reptiles. Its nearest affinities are less with recent saurians than with certain long extinct members of its order. In so far as it is an aberrant, it is also, for the most part, a generalized lizard, resembling lower rather than higher forms. The Hatteria deceives us, for its outward aspect gives little clue to the solution of the riddle propounded by its deeper anatomical peculiarities. For this reason it was at first erroneously classed with the Iguanas, to some of which in habit it sufficiently approximates. But the ways of the Chamæleons, no less than those morphological features which yield so much delight to the pure anatomist, are at once seen to be very exceptional and worthy of note by the ordinary observer. Thus, whether we consider their structure or their mode of life, these reptiles may fairly claim the isolated position commonly assigned them.

Does the sub-order and family of Chamæleons include more than one genus? The late Dr. John Edward Gray, who, during his later years, would seem to have felt a passion for the subdivision of old genera and the institution of new ones, has endeavoured to establish no less than fourteen genera in place of the one usually admitted. It is difficult, if not impossible, to agree with this author's estimates. His thirteen new genera rest on characters which are either paltry in themselves, and perhaps not always of even specific importance, or taken from one sex only. Rightly to classify Chamæleons, we need very extensive suites of specimens preserved in fluid, collected from as many localities as possible, and accompanied by the notes and drawings of intelligent travellers.

A much more competent authority, Dr. Günther, distinguished among living zoologists for his knowledge of the species and genera of cold-blooded vertebrates, has lately proposed a new genus of Chamæleons, which we have no choice but to admit for the present. This genus, *Rhampholeon*, contains one well-marked species (*R. spectrum*), whose strange characters are manifest at a glance (see Pl. iii. figs. 1, 2). It is a small species, with an exceptionally short tail. The male has a total length of thirty-nine lines, the tail being thirteen; the female is thirty-five lines, with a tail nine lines in length.

'The tail is so short that it can serve as a prehensile organ

* See his memoir in the *Philosophical Transactions* for 1867.

in a very subordinate manner only. This defect is compensated by the development of an additional sharp denticle at the inner base of each claw, and of a spine vertically projecting from the flexor side of each finger and toe, which must immensely strengthen the power of the animal for holding on to branches.'*

Dr. Gray's list,† published in 1864, enumerates thirty species of Chamæleons (or an average of two species to each of his genera). More than a dozen others have since been added.

It is very necessary to note, in our study of the variations to which the species of Chamæleons are subject, the secondary sexual characters which they display more strikingly than any other reptiles. Mr. Darwin,‡ in his account of selection in relation to sex, thus treats this division of his subject:—

'In the genus Chamæleon we come to the climax of differences between the sexes. The upper part of the skull of the male *C. bifurcus*, an inhabitant of Madagascar, is produced into two great, solid, bony projections, covered with scales like the rest of the head; and of this wonderful modification of structure the female exhibits only a rudiment. Again, in *Chamæleon Owenii* from the West Coast of Africa, the male bears on his snout and forehead three curious horns, of which the female has not a trace [see Pl. iii. figs. 6, 7]. These horns consist of an excrescence of bone covered with a smooth sheath, forming part of the general integuments of the body, so that they are identical in structure with those of a bull, goat, or other sheath-horned ruminant. Although the three horns differ so much in appearance from the two great prolongations of the skull in *C. bifurcus*, we can hardly doubt that they serve the same general purpose in the economy of these two animals. The first conjecture which will occur to every one is that they are used by the males for fighting together; but Dr. Günther, to whom I am indebted for the foregoing details, does not believe that such peaceable creatures would ever become pugnacious. Hence we are driven to infer that these almost monstrous deviations of structure serve as masculine ornaments.'

Here we may refer to Ford's beautiful figure§ of *C. Melleri.* The snout of *C. gallus*, a small Madagascar species (Pl. iii. fig. 3), 'has a long pointed, flexible appendage, which is covered with large soft tubercles.' In both these species the male only is known, as in the no less curious *C. malthe*, *C. brevicornis*, and *C. globifer.*‖

* *Zoological Society's Proceedings*, 1874, p. 442 and Pl. lvii.

† In his *Catalogue of Lizards*, published in 1845, only eighteen species of Chamæleons are mentioned. See *P. Z. S.*, 1864, p. 465.

‡ *Descent of Man*, vol. ii. 1871, p. 34.

§ *P. Z. S.*, 1864, Pl. xxxii. It accompanies Dr. Gray's *Revision.*

‖ Chamæleons from Madagascar, described and figured by Dr. Günther in *P. Z. S.*, 1879, Part I.

Doubts as to the limits due to variation must check our statements touching the geographical distribution of the Chamæleons. We have not, hitherto, been able to reject many of the reputed species. Of others the precise range remains to be ascertained. Many seem local (confined to restricted areas).

We know no good species which does not inhabit Mr. Sclater's Ethiopian region.* The common Chamæleon is found in Southern Africa, and is, moreover, the only well-ascertained ultra-Ethiopian species. The number of continental is about equal to that of insular species. But few species cross the equator, though the number of such species will probably be increased by the researches of future collectors. Certain it is that south of the equator Chamæleons are more numerous.

Southern (extra-tropical) Africa has six species, besides *C. vulgaris*. Four are peculiar, *C. ventralis* and *C. pumilus* from the Cape, *C. namaquensis* from Little Namaqualand, and *C. melanocephalus* from Port Natal. Two others, which occur at Port Natal, are also tropical, *C. dilepis* from Gaboon and *C. nasutus* from Madagascar. At least a dozen species, in addition to *C. dilepis*, belong to western tropical Africa. Fernando Po has two peculiar species (*C. Burchellii* and *C. Owenii*) and one common to it and Old Calabar. The other western species are continental. *C. gracilis* has the greatest meridional range, extending from Senegal to Angola. South of the equator we also find *C. Capellii* from Benguela and *C. anchietæ* from Mossamedes. On this side of Africa the species seem more numerous north of the equator. The Camaroons yield *C. montium* (Pl. iii. figs. 4, 5) and *Rhampholeon*, the most aberrant of all the Chamæleons. Lastly, *C. Brookesii* quits western Africa, reappearing in Madagascar!

From eastern tropical Africa we have four continental species, *C. lævigatus* from Khartoum, *C. affinis* from Abyssinia, *C. Petersii* from Mozambique, and *C. Melleri*. The first is, probably, further from the coast than any other tropical species. Of insular species, all south of the equator, twenty-one are peculiar, namely, from

Madagascar	15	species.
Madagascar and Bourbon	3	„
Madagascar, Bourbon and Mauritius	1	„
The Comoro Islands	1	„
The Seychelles	1	„

Madagascar has a total of twenty-one species; but two of these are also continental. According to Böttger, Chamæleons make up one-fourth of the saurian fauna of this island, which we know to be equally peculiar as to its mammals. Five new

* Not including Northern (extra-tropical) Africa, which, with part of Asia and Southern Europe, belongs to the Mediterranean sub-region.

species of Chamæleons from Madagascar have been described by Dr. Günther since Böttger's estimate was made. In no other region is the genus Chamæleon so conspicuously represented. It is curious, if true, that the tropical African mainland nearest to Madagascar should be poorer in species than the western coast. Is this due to its greater humidity,* or have we here to deal with an effect of migration, as in the case of the singular reptilian fauna of the Galapagos Islands? †

We find little or no mention of Chamæleons frequenting central Africa properly so called.

Omitting the Cape species and dividing the Ethiopian region by its principal meridian, that of 20° E. longitude, we find only two species of Chamæleons which live on both sides of this line.

The limited distribution of the Chamæleons and the fact that none are known to be extinct ‡ indicate the lateness of their origin. Pliny has called Africa the land of wonders in a sentence approvingly quoted by Linnæus.* Professor Owen, who cites the same passage, has shown that its reptilian fauna in past times was no less wonderful than in the present.

We now return to the common and famous species, which also enjoys by far the widest range. It occurs in Spain (Andalusia), northern Africa, southern Africa, Asia Minor, the Indian Peninsula and the northern parts of Ceylon. The British Museum contains specimens said to have been brought from Singapore and even Japan. Its presence in Ceylon has recently been denied, but the rebutting evidence on this point is indisputable. The occurrence of the Chamæleon in Sicily has been asserted, denied, and re-asserted.

Does the common Chamæleon (like *C. Brookesii*) belong to the list of what Alphonse de Candolle has termed 'disjointed' species? Thus, we find it recorded from northern and southern (but scarcely from intertropical) Africa. This alleged distribution plainly suggests that of the many African species one or more may be varieties of this common form. Have we not here

* For 'west coast' read 'east coast' in paragraph (275) of Sir John Herschel's article on 'Physical Geography,' in the last edition of the *Encyclopædia Britannica.*

† See chap. xvii. of Darwin's *Naturalist's Voyage.*

‡ We do not forget the fossil found at Wyoming of which the following account has been published:—

'CHAMELEO PRISTINUS. Indicated by a lower jaw fragment containing eight teeth in a space of five lines. In every respect it agrees in character with the corresponding part in living species of the genus.'

So noteworthy a conclusion as the existence of Chamæleons in North America during Eocene times must rest on fuller evidence than this passage affords.

a case somewhat similar to that offered in botany by the Cedar of Lebanon, with its western and eastern outliers, the Atlantic Cedar and the Deodara? Again, Dr. Gray cites no localities intermediate between Asia Minor and Hindostan. The Chamæleon of our childhood, from 'Arabia's wilds,' as narrated in Merrick's poem, is not, according to Dr. Gray, the common form but a distinct species, *C. auratus*. Such questions should not be undecided.

The Chamæleon is often mentioned but little cared for by the vulgar, who regard the creature with the misplaced wonder of contented ignorance rather than with the intelligent curiosity which it deserves. It can hardly be called a favourite, though among the cold-blooded vertebrates there is no other animal so well fitted to be made a household pet. Our knowledge of its structure and actions is still far from complete, yet very many naturalists have studied it. A long list of essays specially devoted to its elucidation might easily be cited; and many allusions are made to it in more general works, with titles which would scarcely lead us to expect such references. Two lines of inquiry need to be followed up, that we may trace what remains to be ascertained of the Chamæleon's nature and history. First, its several parts, the eye only excepted, have not hitherto been minutely analysed with those modern aids to research which are now so accessible. In the second place, the functions of its muscular and nervous systems have never been duly investigated by competent physiologists, availing themselves of the resources of experimental methods.* Much might be learned in this way, even though we should curtail our studies from an unwillingness to subject the living animal to pain. Therefore, the life of the Chamæleon, as contemplated by men of science, still remains in many respects a mystery.

The Chamæleon may from time to time be bought and kept in captivity. Care should be taken to supply it with plenty of flies, crickets or such other insects as can be had. (A fly-trap of honey or syrup may be used to save trouble.) It should be lodged in a properly ventilated glass case, some thirty inches in length breadth and height, furnished with a suitably branched shrub or fragment of a tree. A warm temperature should be maintained about it. Lastly, it should be allowed to relieve its thirst. The necessity of so doing is well shown by Brehm,† who carefully studied Chamæleons when residing in Alexandria. He procured during the summer a large number of healthy specimens, more than a third of whom died after a fortnight's captivity. The remainder were very languid, of a dull uniform

* Some use, it is true, has been made of these methods in the study of the Chamæleon's changes of colour.

† See his *Thierleben*, seventh volume of second edition, 1878.

grayish yellow colour, and careless of the food abundantly supplied them. Brehm now tried the experiment of treating his pets to an artificial shower of rain. Like magic, they revived. Their skin reassumed its more vivid and changing tints; they moved their sluggish limbs, going from leaf to leaf in quest of the grateful moisture, and, displaying with increased energy their insatiable greed for prey, soon appeared to be in better health than ever.

Those who have seen Chamæleons in life would laugh at us for attempting to describe their form. Those who cannot view the living animal will learn more from the excellent (though uncoloured) figure of a group of Chamæleons in the work of Brehm than from any written description or even, we might add, from the inspection of preserved specimens. We must, however, say something which may induce our readers to study these animals more closely, and we make, therefore, the following attempt to represent the Chamæleon's most characteristic features.

The head is large, compared with the rest of the body, and though relatively short, is wide and very deep. In general form it is angular, with a high occipital crest, from the raised hinder apex of which a ridge-like wing descends on either side, then arches over each orbit, and finally stretches forward to meet its fellow just behind the rather blunt muzzle. The gape is very wide, the under jaw capacious. Ths nostrils lie far forwards, but are not very close. There are no external ears.

The neck is short and stiff, appearing externally as little more than a fold between the head and the narrowed shoulders, from which the whole body slopes gradually backwards.

The trunk proper, compressed laterally, is singularly lean, and is sharpened along the middle line both of the back and belly, the ventral ridge being continued on the chin.

The tail also is much compressed, but is rounded beneath. It is prehensile, and usually twisted round some support. It constitutes more than half the total length of the animal, which is about ten inches.

The slim nearly cylindrical limbs, not swollen in any part and much longer than the trunk, remind us of those of Cassius or Don Gonzales Pacheco. They end in hands and feet stouter than themselves, with their digits so arranged as to grasp securely the branches on which the animal rests. In each hand, the thumb, index and middle fingers, united by a membrane as far as their nails, are directed inwards; while the two other fingers, likewise united, are turned outwards. In each foot, the first and second toes are connected and turned inwards;

they are opposed to the three outer toes, connected in the same way.

The Chamæleon does not grovel like other reptiles. Its hips and shoulders are so disposed as to allow the limbs to sustain the trunk at a notable height above the branch which supports it.

When not pursuing its prey the Chamæleon maintains an almost corpse-like stillness. But its rapidly shifting eyes, moving independently of each other and often glancing in different directions, contribute much more than the creature's changes of colour to enhance the strange weirdness of its aspect. Nor is this effect lessened, when it begins to stir, by its very stealthy and deliberate movements. Having sufficiently neared its prey, it pauses while it takes aim; and the body in general is motionless as the tongue escapes from its mouth with incredible velocity.

These strong contrasts of motion and rest, its painfully gaunt form unrelieved by any amount of gluttony, and its marvellous fitness to do the work of its life are the chief sources of our interest in the Chamæleon. As a fly-catching machine it is perfect. What seem defects in its organization are truly the reverse. We repeat that the Chamæleon's feebleness, rightly understood, must be regarded as operating in its favour. If it moved its legs quicker, its eyes or its tongue slower, it could not secure its prey with such fatal accuracy. We have seen ten minutes elapse between the first sighting and the final capture of a large bluebottle-fly by a captive Chamæleon. During this protracted interval, as the animal with persistent caution stole gradually upon its victim, alternately raising and putting down one leg after another, no doubt of its ultimate success could be entertained.

In Merrem's arrangement of lizards the Chamæleons constitute the group of *Prendentia*, on account of their grasping limbs. They are more frequently named *Vermilingues* or *Rhiptiglossi*, from their peculiar tongue.

The Chamæleon's skin is, for the most part, not scaly in the ordinary sense, but rather soft and extensible. Small distinct tubercles of uniform size serve to strengthen it. The dorsal ridge is minutely serrated. Along this and the ventral ridge, as well as on the head and limbs, the tubercles are closer, flatter and more scale-like.

The common use at all times of the word Chamæleon in metaphor, its application by the ancients to certain plants and by the moderns to a well-known mineral, show that the changes of colour to which it is subject have attracted much more attention than its other peculiarities. In this respect, however,

the Chamæleon enjoys a pre-eminence which it scarcely deserves. For many lizards and other animals possess the same property, if not in the same degree. We may mention more particularly the cuttlefishes among invertebrates, and among true fishes the sticklebacks. A large number of similar instances might be cited.

We must here distinguish changes due to the presence of special pigment-cells, or 'chromatophores,' from diversities of colour depending on true variation, sex, age, seasons of the year (notably in animals which moult) and the more transient results of altered states of the circulation, as in blushing or the reverse.

When chromatophores are present they are affected by physical influences, such as light, heat, contact of foreign bodies, and other varying external conditions; they are also subservient to emotional causes.

Chromatophores occur in several reptiles, batrachians, fishes, mollusks, crustaceans and insects. They are deep-seated tegumentary cells which contain a dark pigment. According as they approach or recede from the surface of the skin, they modify the tints of the semi-transparent more external layers.

But how is this movement effected? It is not the result of muscular action as some have supposed, for the skin has no muscles which can thus rearrange its pigment-cells. We hold the opinion of Leydig, who is supported by von Siebold, that the chromatophores are themselves capable of contraction. The contractile substance would seem to be situate chiefly in their outer portions. The pigment itself is not diffused but granular. It has no inherent contractility, however it may be disposed in relation to the true contractile substance.*

That the nervous system governs the movements of the chromatophores may be admitted. Bert, who has made experiments on Chamæleons to settle this question, asserts the presence of two distinct sets of nerves, by which the alternating changes of the chromatophores are directly excited. He even assigns to these nerves their appropriate centres. But these experiments should be repeated. Perhaps they prove too much, for we need more positive evidence to disprove the hypothesis that the retrocession of the chromatophores is essentially a passive movement.

The opposite sides of the same Chamæleon may be differently coloured. Dead and sickly Chamæleons are paler than others. So, likewise, are Chamæleons kept in darkness. We cannot follow Bedriaga in his efforts to ascribe the dark tints

* Harting has made the surprising statement that the peculiar pigment-cells of a cuttle-fish (Loligo vulgaris) have a coloured wall enclosing a cavity filled with soft contractile protoplasm.

(whether temporary or permanent) of these and other lizards to the sole influence of light, acting directly on the deeper cells of the skin and causing them to assume a more superficial position. Leydig has sufficiently disposed of this explanation. Our space does not allow us to cite the details of his very interesting and convincing observations.

All the actions of the Chamæleon, except during the breeding season, tend to the procuring of food. The tongue is the prehensile organ used for this purpose. A Chamæleon projecting its tongue may truly be designated the entelechy, or perfection, of the living animal.

When not thus employed, the tongue lies concealed in a special depression of the floor of the mouth. We must therefore distinguish its retracted from its extended condition. Brehm rightly indicates a third, or intermediate, phase, in which the tongue, previous to being ejaculated, is loosened and pointed towards its prey.

The tongue consists of two principal regions, a proximal and a distal. The former, or so-called 'worm,' is exceedingly extensile, but is much shortened in the retracted condition. The latter is terminal and club-shaped, preserving in its alternate states nearly the same dimensions. Milne-Edwards distinguishes a basal division, behind the worm, which may rather be said to consist of the hyoid muscles. These are very complicated. A full technical description of them would be out of place in the present paper.

The worm is not inaptly so termed, whether we view it in its retracted or extended state. In the former, it is much corrugated transversely, and is about as long as the 'club;' the whole tongue having an average length of an inch and a half. When fully stretched out, to an extent of some five inches, the worm is nearly smooth. A trifling fold still distinguishes it from the club, into which it passes by a gradual enlargement.

The club is bilobed, with an upper and a lower 'lip.' The extremity of the latter projects in front of the tongue, and its ventral aspect is plainly more elevated than that of the club lying behind it, from which it is marked off by an indentation. The upper lip, or 'dome' of Mr. Salter, is itself shaped like a tongue in miniature, having a raised dorsal surface, the gently narrowed hinder portion of which descends rather abruptly to join the club proper. From this part to its anterior end the dome is more than half as long as the club. The shallow fissure between the two lips is dilated into a funnel-shaped cleft by their separation when the tongue is thrown forwards.

A glutinous secretion lubricates the club, which has minute

glands of its own. But other glands moisten the tongue, as it lies in the recess of the lower jaw.

The tongue is a tube, the axial cavity of which is occupied by a slender cartilage, the glosso-hyal or ento-glossal, supporting nearly the whole length of the organ in its state of rest. The soft parts of the tongue consist of (1) epithelium; (2) pigmentary and submucous tissue separated by loose interstitial connective tissue from a fibrous sheath, and (3) longitudinal muscles, besides nerves glands and vessels. There is also an inner fibrous sheath with a smooth free surface, which glides over the glosso-hyal cartilage. The club has peculiar muscles, well described by Zaglas. The existence of minute smooth intrinsic muscles within the substance of the worm is still disputed.

It is probable that four sets of muscles successively promote the extension of the tongue, namely, (1) certain of the hyoid muscles, (2) the longitudinal muscles, (3) the smooth muscular fibres of the worm, and (4) the muscles of the club. Other hyoid muscles retract it. To the same muscles, however, different uses have been assigned by different observers. It has even been conjectured that the extended condition should be regarded as the more passive state of the organ. Others consider the protrusion of the tongue as a sort of erection; but such vascular turgescence must here play quite a subordinate part. Nor can the mere elasticity of the sheath effect much. We cannot here discuss these diverse explanations. The whole subject demands fresh researches. As Milne-Edwards has well said, the mechanism of this movement has not yet been explained in a satisfactory manner.

Though very carefully aimed, the tongue darts from the mouth as if its previous training thoroughly suited its own spontaneous energy. Perrault erroneously supposed that it was coughed out by air driven into it from the lungs. Bibron has graphically compared its expulsion to the act of spitting. Pagenstecher, on one occasion, saw a Chamæleon eject its tongue with such force that the animal lost its hold in consequence, and tumbled off the tree on which it was resting.

From the tongue of the Chamæleon we naturally pass to its eyes. For by these the prey is first perceived, and if we could trace the intermediate changes which take place in the nervous system we should then be in a position to understand how the tongue receives the orders to do its work.

The eyes are relatively large. Each as a whole is nearly spherical, the equatorial slightly exceeding the axial diameter ($8\frac{1}{2}$: 8 millimeters). But what may be termed the morphological equator of the eye, corresponding to the border of the

sclerotic in front of the retina where the corpus ciliare and ciliary muscle are placed, is of no great extent. It gently rounds off into the region behind it, in a manner not conspicuous externally. The anterior portion of the eye is more strongly prominent.

The basal moiety of the sclerotic is exceptionally small. It is constituted by a round cartilaginous disc overlain by a layer prolonged from the much larger fibrous portion. This disc does not reach the optic nerve. The anterior zone of the fibrous moiety, surrounding the lens, is strengthened by a ring of peculiarly curved thin bony plates which are sunk in its substance.

The choroid is very thin, but is notably thickened posteriorly throughout the region subtending the yellow spot. A somewhat conical projecting pecten, about a millimeter in depth, covers the place of entrance of the optic nerve. The corpus ciliare is broader than the iris and pupil taken together. The muscles of the iris are extensive, as in birds; from the marginal sphincter fibres stretch backward towards the ciliary border, and behind these is a less powerful radial dilatator. The iris of the dead Chamæleon is half as wide as the pupil. It lies in complete contact with the lens. Its brilliant anterior surface has a dark background. Insignificant prominences replace the ciliary processes, anterior to which occur exceedingly weak meridional flutings. On the outer side of the corpus ciliare the ciliary muscle (*tensor chorioideæ* of Brücke) arises from a conspicuous lamina of connective tissue, which reflects light and serves to support the cornea. The relative diameter of the cornea is less than in any other vertebrates except the turtles.

The anterior chamber of the eye, containing the aqueous humour, is remarkable for its slight depth.

The lens, on the other hand, is strongly convex.

But of all parts of the eye the retina is most worthy of study, because of its large yellow spot. This, the region of exact vision, occupies the hinder pole of the retinal concave, and displays a central pit surrounded by a far extending zone, throughout which the retina is much thicker and beautifully modified in its minute structure. Especially modified is the so-called percipient layer.

The corresponding region of the human eye shows this layer to be furnished in the fovea proper with cones only. The yellow spot around it has many cones and but few rods, while in the remainder of the retina the rods greatly outnumber the isolated cones. Of two equal retinal areas that which has more numerous (and therefore more slender) cones permits more precise visual discrimination. Heinrich Müller dwells on the

following points of difference between the percipient layer of the Chamæleon and that of man.

1. The Chamæleon has no rods, but cones only.
2. The foveal cones in the Chamæleon are remarkably thinner than in man.
3. The (absolute) length of the foveal cones in the Chamæleon, notwithstanding its smaller eyes, is more conspicuous than in man.
4. The difference in the diameter of the cones, both in the peripheral and central regions of the retina, is greater in the Chamæleon.
5 The region corresponding to the human yellow spot is more extensive in the Chamæleon.

On the whole, sums up H. Müller, if we compare the human eye with that of the Chamæleon, the reptile has altogether the advantage.

Outside the bulb of the eye, but within the large though shallow orbit, the optic nerve, which is here remarkably long, makes a complex loop. It bends downwards, outwards, and again upwards (or even inwards, in certain positions of the eye), previous to its insertion.

Retractor muscles of the eye appear to be absent.

There is a large Harderian gland at the anterior angle of the eye, although the nictitating membrane is rudimentary. The lachrymal gland is small.

The two eyelids of man are represented by one great circular curtain, drawn over nearly the whole periphery of the protruding bulbus and circumscribing a small central orifice. A bony plate lies in the lower moiety of this lid. Beneath the skin of the lid, which resembles that of the general surface, is spread an extensive orbicularis muscle.

The free surface of the conjunctiva, very distinct from the adjoining lid, is also of unusual extent, reaching as far back as the equator of the bulb. The extraordinary mobility of the Chamæleon's eyes, in which it far surpasses all other vertebrates, is much aided by this arrangement, to which the curious curvature of the long optic nerve also contributes.

Thus, whether we consider the eye itself or its appendages, we have to do with an apparatus which is without parallel in the animal kingdom.

The male of the common Chamæleon differs but slightly from the female. He is known by his occipital crest, which is longer and higher, and by the shorter fold occupying the region of the neck.

The female lays a heap of sphæroidal eggs, grayish-white in tint, with calcareous, very porous shells. Oviposition has been

observed only in captivity. No one seems to have witnessed the hatching of the eggs, or to have determined the period of incubation. Brehm found that many females, 'even the strongest and healthiest,' die before or soon after the breeding season is over. He gives an interesting extract from Vallisneri, who noticed that one of his captive Chamæleons was for some days restless on her perch. Thence she slowly descended, moved about for a while, and at length paused in a corner of the floor of her box, which was covered with hard earth. In this she made a hole with one of her forepaws. For two days she worked, digging a pit about ten centimetres wide and fifteen deep. In this pit she now laid more than thirty eggs, and then, as she retired, pushed back the earth with her hind-feet, treating the heap of eggs as cats do their dung. Finally, she covered the heap with straw, dried twigs, and withered leaves.

That the Chamæleon bears living young is often untruly said. It is well known that in many so-called viviparous reptiles the egg is detained in the oviduct and there developed. What is thus normal in these may possibly occur as a rare (or pathological) exception in the Chamæleon. But full proof is wanting.

EXPLANATION OF PLATE III.

(*All the figures are of the natural size.*)

FIG. 1. *Rhampholeon spectrum*, Günther, from the Camaroon Mountains. Male.

FIG. 2. The same. Female.

FIG. 3. *Chamæleon gallus*, Günther, from Madagascar. Male.

FIG. 4. Head of Male *Chamæleon montium*, Günther, from the Camaroon Mountains.

FIG. 5. The same. Female.

FIG. 6. Head of the Male *Chamæleon Owenii*, from Fernando Po.

FIG. 7. The same. Female.

THE NEW CHEMISTRY, A DEVELOPMENT OF THE OLD.

By M. M. PATTISON MUIR, M.A., F.R.S.E.

In a paper published in this *Review* (January 1878), I endeavoured shortly to summarize the more important differences between that system of chemistry which was founded on a so-called equivalent notation, and the modern, or atomic phase of the science.

The general conclusion to which that summary led was, that the old chemistry was empiric, whilst the new is scientific; but, as was there remarked, empiricism precedes science: science is the natural development of empirical statements, and is not to be regarded as entirely a new departure.

Believing, as I do, that the old and new chemistry are essentially opposed in their methods, I nevertheless am certain that the germs, at least, of many of our modern chemical theories are to be found in the statements, and even in the hypotheses, of the workers of half a century since: and in the present paper I propose to trace, in a little detail, what I believe to be a correct outline of the development of two of the more important theories of modern chemistry.*

The chemical views most in vogue before the strictly modern epoch, were founded more on considerations of the composition of compounds than on the actions of these compounds. Dumas introduced wider views by recalling the attention of chemists to the fact that in order to frame even a tolerably complete system of classification, an answer must be given to the question, 'What does this substance do?' no less than to the other question, 'Of what is this substance composed?'

* In the paper referred to, I briefly sketched the history of the development of the older doctrine of 'Equivalents' into the modern hypothesis of 'Valency.'

But if we go back to the time before Lavoisier and his associates, we find that the system then predominant in chemistry was founded almost entirely on the reactions, and but to a very small extent on the composition of chemical substances.

Chemists then busied themselves continually with studying processes of chemical change; only they contented themselves with qualitative knowledge, and hence their hypotheses were for the most part extremely vague and their facts disconnected.

John Joachim Becher, born about 1630, seems to have been the first to weave together the scattered chemical facts and guesses into a consistent general theory, which was subsequently augmented and defined by Stahl (1660–1734).

Looking at the wonderful changes produced in substances by the action of chemical force, the question arose, what happens when a body undergoes chemical change? and as burning or combustion was perhaps the commonest of all chemical changes, the question became narrowed, and chemists eagerly sought for an answer to the query, 'What happens when a chemical substance burns?'

In those days natural phenomena were referred to the presence of 'principles' or 'essences' in the matter exhibiting the phenomena. A new principle was added to the list; and the question was supposed to be solved by saying that combustible substances are characterized by richness in *Phlogiston*, (Gr. Phlogizō = burn, or inflame), and that when they burn they lose this principle, so that the burnt substance, or calx, consists of the original substance *minus* Phlogiston.

The Phlogisteans seem to have regarded their hypothetical principle as a modified form of fire, as fire confined in a material substance; but as they gave no definition of fire, beyond saying it was one of the four elements, it was scarcely to be expected that they should define Phlogiston.

By restoring Phlogiston to the burnt substance, said the theory, the original matter is regenerated. Some substances, *e.g.*, charcoal, are especially rich in Phlogiston, and metallic calces may be converted into metals, *i.e.*, may be unburnt, by heating them with charcoal.

Thus the Phlogisteans regarded the phenomena which they studied in a purely qualitative manner: they asked only, What does this or that substance do under given conditions? not being aware that a full answer to this question can only be given, when the other question—*How much* of some given effect is produced by a given quantity of this body under stated conditions? had been answered.

The introduction and use of the balance carried the day in favour of those who opposed Phlogistic views. If a substance

loses something when it burns it *must* weigh less than before burning—as a fact it weighs more—therefore it has not lost but gained something.

'Nay,' replied the Phlogistean, 'it has lost something, but the weight of this something can only be expressed by a negative quantity.'

'But a something with such properties is an absurdity,' replied the opponent, 'therefore it has no existence, and therefore your theory is utterly false.'

The anti-Phlogistean triumphed, and the principle of levity was banished from chemical science.

But the principle returned in a modified form. Lavoisier, who opposed the Beccherian theory of Phlogiston with signal success, himself propounded a theory of the constitution of solids, liquids, and gases, in which the 'subtle principle' '*caloric*' played an important part. Lavoisier regarded oxygen, as what he termed 'concrete oxygen' *plus* a something—caloric; indeed he appears to have looked on all substances in the concrete state as solids, and to have supposed that the addition of a certain quantity of caloric to these caused them to become liquids, whilst the addition of a further quantity of caloric produced gases.

Thus chemists seemed obliged to imagine a something in addition to the gross or ponderable matter of which bodies are composed, in order to account for the properties of these bodies. As science has advanced she has been able to define what this something is, at least, she has defined it more clearly than the older workers could do.

I have said as science has advanced she has defined the unknown something; but it should be remembered that that wonderful book, which contains—according to the greatest authorities—the germs of all our modern advances, was written sixty years before Lavoisier's time. Sixty years before the apparent overthrow of the theory of Phlogiston, Newton had laid the foundations of the science which was to reveal the true lineaments of that Unknown whom the Phlogisteans ignorantly worshipped.

We have learned to extend the meaning of the word *thing*—we speak of 'the power of doing work' as a measurable and definite thing—although not as matter: and we know that when a body burns it loses a certain amount of this power of doing work, or, as it is more shortly put, of energy.

As usual it is a question of words. The older workers could not define Phlogiston; we are able to define energy, and therefore, we can see clearly where they saw but darkly. Chemistry now acknowledges that the properties of a compound are not only determined by the composition of the

matter of that compound, but by the amount of energy associated with that collocation of matter. She has been able to point out many instances of compounds composed of the same matter, but possessed of different amounts of energy, and, at the same time, of very different properties. And moreover, chemistry aided by physics, has concluded that the properties of a body 'are dependent on the variations of the energy of the body, and not on its total value,' and therefore that 'it is unnecessary, even if it were possible, to form any estimate of the energy of the body in its standard state.' (I quote from that remarkable little book of the late Professor Clerk Maxwell, *Matter and Motion*.)

Whenever science made the advance from the vague conception of 'principles' and 'imponderable matter' to the definite conception of 'mass,' 'motion,' and 'energy,' she was able to recognize the truth which lurked under the cumbersome and inexact nomenclature of the Phlogistean chemists.

I have said that, as usual, the dispute between the Phlogisteans and their opponents was proved to be a question of meaning of words: as usual, also, subsequent research showed that while both were wrong, both also were right.

Composition is important, but composition is not all. The burnt body has properties differing from those of the unburnt body, because it has lost a certain amount of 'the power of doing work;' but it has a less power of doing work because it is possessed of a structure different from that which it possessed before. Composition and properties, energy and structure, are closely connected: to determine the exact relations existing between these, under stated conditions, is still the fundamental problem of chemical science.

We can define Energy: the Phlogisteans could not define Phlogiston. But in the ethereal philosophy of the future will it not be said of the present workers in science that they could not define Ether, but even spoke of it at times as 'not gross nor ponderable matter?'

The theory of Phlogiston was continued and developed in the theory of Caloric: the theory of Caloric is vastly extended, simplified, and rendered definite in the theory of Energy: and the theory of Energy seems destined to be largely extended by the Ethereal theory now in its infancy.

Mankind has until lately been content with space of three dimensions, but the bolder and more dashing spirits among the mathematicians have dared to look forward to a better world than this where they may revel in space of four dimensions. What a strange world must that be! what a fearful place for a mathematical examination, when we remember that the inhabitants thereof — if there be inhabitants — may turn

spherical hollow balls inside out without tearing or breaking them!

While we look forward to the future of science with hope, I think we ought not to look back on the former workers without respect.

But I must pass on to consider the second of the great theories which have paved the way for the doctrines of modern chemistry. The germ of the modern ideas of substitution, valency, atom-linking, &c., is, I believe, to be found in the pure dualism of Berzelius, and moreover, the influence of the dualistic ideas of that great chemist seems to me easily traceable in the essentially unitary system of modern chemistry. The chemistry of Lavoisier centred around the wonderful substance whose properties he so carefully studied. The teaching of the great founder of modern chemistry was saturated with ideas suggested by the study of oxygen. The compounds of oxygen were divided by Lavoisier into two groups, bases and acids: when these reacted chemically, a salt—that is, a body made up of base and acid—was produced. Berzelius developed these ideas until he had constructed a complete and beautiful theory, viewed in the light of which all compounds were of analogous structure. Every chemical substance was made up, according to the Swedish chemist, of two parts; these parts might themselves be composed of simpler parts, or they might be truly elementary. The two parts of a compound were respectively endowed with positive and negative electricity. When two bodies combined the positive electricity in one neutralized the negative electricity in the other; hence the phenomena of light and heat noticed in chemical combination. An element might contain an absolutely greater quantity of positive electricity than another and nevertheless belong to the electro-negative series of elements: thus sulphur and oxygen readily combine to form a substance which, when dissolved in water, yields an acid. But oxygen and sulphur are both electro-negative elements. Berzelius supposed that sulphur contained a large quantity of both electricities, the negative predominating. When this element combined with oxygen, the positive electricity of the sulphur was supposed to be neutralized by the negative electricity of the oxygen, so that the negative electricity of the sulphur was concentrated or rendered more apparent. The affinity between oxygen and silver is less than that between sulphur and oxygen, because, said Berzelius, silver contains mainly positive electricity, but a smaller quantity than is found in sulphur. The product of the union of oxygen and sulphur, *i.e.*, of oxygen with an electro-negative body, belongs to the class of acid oxides; the product

of the union of oxygen and silver, *i.e.*, of oxygen with an electro-positive element, belongs to the class of basic oxides.

If this view of the composition of oxides were granted—and a most ingenious and plausible theory it was—why should we not proceed a step further and say that an acid acts so readily upon a base, because in the first, negative electricity predominates, while the prevailing electricity in the latter compound is positive?

And in further support of this view could it not be experimentally demonstrated that when a salt, such as sulphate of sodium, is decomposed by the electric current, the soda goes to the negative pole, whilst the sulphuric acid appears at the positive pole? The experiment of decomposing a solution of sulphate of sodium was frequently performed, and the fact, that if the solution were coloured with litmus, that portion around the negative pole retained its blue colour, whilst that around the positive pole became red, was regarded as conclusive evidence of the dualistic structure of the salt operated upon.

But about the year 1834 Dumas told the chemical world that chlorine was capable of 'laying hold of the hydrogen of certain bodies and replacing it atom for atom.' If this be so, said Berzelius, the compound formed must differ essentially from that from which it is derived. Chlorine is an electro-negative element, and if it enter into a compound in place of the electro-positive hydrogen, the original compound and the new compound can present no points of analogy. The theory seemed correct, but unfortunately the chlorinated body did present very marked analogies with that from which it had been produced. Berzelius attempted many explanations, invented many new compound groups of atoms, which should be supposed to enter into the composition of the new bodies discovered by Dumas; but his electro-chemical theory was doomed. It was gradually abandoned by most chemists, and the substitutionists carried the day.

Berzelius had largely availed himself of certain facts, which showed that, in series of reactions, it was sometimes possible for a group of dissimilar atoms to remain intact, to move about, so to speak, from one compound to another without falling to pieces. Reasoning on these facts, he constructed formulæ for all compounds, which formulæ were made up of two parts, or radicles. The idea of compound radicles was thus closely associated with the dualistic theories of the Berzelian school. The new school, led by Dumas, finding dualism insufficient to explain many weighty facts, naturally waged war against the fundamental conception of compound radicles, but they were soon obliged to accept the essential truth of the theory which

they at first opposed. Liebig and Wöhler's research on oil of bitter almonds led to the discovery of a number of compounds, exhibiting many general analogies, which could best be explained by supposing the existence in each of a compound radicle, or group of atoms. When it became necessary once more to adopt the idea of compound radicles, the theory of substitution was found to be strengthened, not weakened, thereby. Many reactions were made clear by supposing that an element might be substituted by a group of elementary atoms, by a compound radicle. But in adopting the idea of compound radicles the substitutionist yet maintained that the chemical compound was a distinct whole, made up of parts he admitted, but, nevertheless, having these parts so modified and merged in one another that the resultant acted as a homogeneous compound. Thus when the new school likened the ethers to the metallic oxides, they did not mean to assert that the molecule of ether was composed of two parts, ethyl and oxygen, held together by electric bonds, and ready to part company without difficulty; nor, in asserting that ether was one substance, and not a dualistic system, did they deny the existence of a structure within the molecule of ether. They admitted the existence of a closer relationship between the atoms of carbon and hydrogen constituting the group ethyl, than between these atoms and those of oxygen, and they generalized the reactions and analogies of ether, by saying that it might be regarded as sodium oxide in which both sodium atoms had been substituted by two compound atoms of ethyl. Berzelius had himself likened the ethers to oxide of potassium, and by doing this the great apostle of dualism had paved the way for the advance of the unitary theory.

That portion of the dualistic doctrine which was embodied in the theory of compound radicles was adopted by the unitary schools, but adopted in a modified form: the effects of this modification were not long in making themselves felt.

Berzelius, in his later works, had been ready to give a dualistic formula to any compound without stopping to inquire into the facts known about that compound: he had tended to forsake the only true scientific method, and to substitute the vagaries of his fancy for the facts of nature. The new school averred that 'compound radicle' was an expression generalizing a class of facts; that the reactions of bodies were most simply explained by supposing that when acted on by chemical force the little parts of these bodies behaved as having a definite structure; and that therefore the formula of a given body might be written as containing different compound radicles under different conditions.

The fault of the old chemistry was that more attention was

paid to symmetrical formulæ than to reactions; the merit of the new consisted in bringing the student once more back to nature.

And the appeal to nature was answered and answered abundantly. The new conception of compound radicles was rich in results; from it there was developed,—first, the theory of types, and subsequently the wider theory of valency, which has led to that of atom-linking, and these in their turn have reacted on the older and more fundamental notions of the science, and have given a new meaning to such terms as 'chemical' and 'mechanical actions,' 'compounds' and 'mixtures,' &c., while, at the same time, they point the way to the chemistry of the future when we shall have gained a definite conception of the inner mechanism of the molecule, and of the laws which regulate the combinations of molecules in groups, and the decompositions of molecules with subsequent formations of new atomic systems.

Let us shortly examine these ideas. If sodium be thrown on to water caustic soda is produced, a substance made up of hydrogen, oxygen, and the *simple radicle* sodium; by another reaction a substance can be obtained, consisting of hydrogen, oxygen, and the *compound radicle* nitryl, (NO_2). These two bodies have analogous formulæ, Na OH and (NO_2) OH, they may both be regarded as derived from water, H HO, by the replacement of one-half of the hydrogen by a radicle; in one case by Na, in the other by NO_2 Again the whole of the hydrogen in water may be replaced by sodium, with production of the compound sodium oxide, Na_2O; but in many of its reactions this compound is the analogue of common ether, which is also a compound of oxygen with a (compound) radicle *Ethyl*, and has the formula $(C_2H_5)_2O$. Now these substances, Na OH, (NO_2) OH, Na_2O, and $(C_2H_5)_2$ O, both on account of the methods by which they are produced, and because of their general reactions, may be classed together as derivatives of water, or may be said to belong to the *water-type*. Similarly, other types have been instituted, and large groups of compounds have been brought into the same class as being all referable to one parent type. This step in advance is evidently an outcome of the theory of compound radicles: without that conception a system of classification by types would have been impossible.

But it was found that while such compound radicles as C_2H_5 or NO_2 were capable of replacing but one part by weight of hydrogen in water, other compound radicles, such as CO or C_2H_4, were capable of taking the place of two parts by weight of hydrogen. Comparing together these two sets of radicles, it might be said that $CO = 2\ NO_2$ or $C_2H_4 = 2\ C_2H_5$, so far as the power of combining with hydrogen was concerned. This

conception of binding power being extended to the elements, and being deepened and widened by laborious experimental researches, led to the general theory of valency, which included in itself the essential features of the older doctrine of equivalents.*

Having thus gained the conception of a definite binding power as applicable to elementary atoms or groups of atoms, it followed, as an almost necessary deduction, that the smallest parts of chemical compounds which existed as distinct chemical entities, *i.e.*, the molecules, must have a definite structure: that the parts (atoms) of the little systems must be arranged in accordance with the valencies, or binding powers, of these parts.

Hence, given the number of atoms in a molecule, and the valency of each atom, it became possible to calculate the number of different arrangements of these atoms which could be produced, and careful experiment has often succeeded in preparing all the different, theoretically possible, compounds. The difference of properties of such compounds, *i.e.*, of compounds the molecules of which are constituted of the same number of the same atoms but differently arranged, is attempted to be indicated in the 'structural' or 'rational' formulæ of modern chemistry.

Berzelius spoke of compounds composed of parts held together by mysterious bonds: the idea survives in these structural formulæ of to-day, only we are now able to define what we mean by the smallest part of a compound having a chemical existence, and we have gained certain generalizations which enable us to trace with some degree of accuracy the relationships which exist between the inner parts of these smallest chemical wholes. We appear to be now fairly embarked in the prosecution of molecular dissection, and our chief guide is the theory of valency, itself a development of the dualistic chemistry.

Each elementary atom, I have said, seems to have the power of directly binding to itself a maximum number of other atoms; but it would further appear as if the groups of atoms thus produced had also a certain binding power, but this more indefinite than the atomic binding power, and very variable under different physical conditions. This atomic binding power appears to have a fixed maximum value, but not always to reach the maximum. What is the exact way in which the binding power or valency of the elementary atoms is influenced by definite changes in physical conditions? This is one of the most important unsolved problems of general chemistry.

Then, again, granting the existence of an inner structure to

* See former paper, Jan. 1878.

the molecule, granting that groups of atoms do exist in the molecular building, does the fact that in a certain reaction certain atoms are withdrawn as a group, prove that these existed in the form of the same group in the original molecule? In other words, do our structural formulæ express the relative collocation of atoms within the molecule while the molecule is unacted on by extraneous force, or do they merely roughly represent the condition of things when the molecule is in a state of strain, because of the stress between its parts and those of another molecule, or molecules, brought within its sphere of action? Here is another question which can only be answered after much experimental evidence has been accumulated. Now these questions, I make bold to say, are the direct outcome of the dualism of Berzelius, modified by the unitary chemistry of Dumas and his followers.

If we glance back on the development of the two theories, the course of which I have endeavoured to outline, we find that both began with a purely qualitative study of reactions, but that it was only when to this had been added the careful use of weights and measures, that any solid advance became possible. Further, we find that the older theory was founded chiefly on a study of reactions, whilst that which was broached after the time of Lavoisier was founded most largely on a study of composition. With the Phlogisteans *function* was of paramount importance; with the Dualists *composition* was all. The modern theories, which have been developed from these, have attempted, with varying success, to combine both considerations. And if we examine the latest advances of theoretical chemistry we still find it at work on these two lines of advance. The composition of chemical compounds is studied by the majority of chemists; but the general laws of action of chemical force itself have of late received most important elucidation.

Again, if we look to the 'lines of advance along which dynamical science is working its way to undermine, at least, the outworks of chemistry,' we can distinguish two, essentially the same, lines as were used by the two classes, whose theories I have dealt with in this paper. 'One is conducted by the help of the hypothesis that bodies consist of molecules in motion, and it seeks to determine the structure of the molecules and the nature of their motion from the phenomena of portions of matter of sensible size. The other line of advance, that of Thermodynamics, makes no hypothesis about the ultimate structure of bodies, but deduces relations among observed phenomena by means of two general principles, the conservation of energy, and its tendency towards diffusion.' (Clerk Maxwell: *South Kensington Science Conferences*, 1876, p. 145.)

I have thus sought to substantiate the claim of the new chemistry to be a development of the old. I believe that if this claim is granted, the conclusion to be drawn must be, not that the old is better, but that to return to that which is admittedly an early stage of development would be to misread all the teachings even of the old chemistry itself.

One general lesson may surely be deduced from what has been said, and that is, the continuity of science. Science proceeds by gradual developments, each dependent on that which went before. She may frequently be obliged to review her past progress, and even, in the light of freshly acquired knowledge, to alter what once appeared to be well-established generalizations. But if any generalization of science be founded on experimentally authenticated facts, it always survives, although not necessarily in its commonly accepted form. The Phlogistic theory was absorbed in, not entirely contradicted by, the Caloric theory of Lavoisier, which in its turn was merged in the fuller and more definite modern doctrine of Energy. The dualism of the Swedish school appeared to be absolutely overthrown by the followers of Dumas, but the theory has survived, and, modified to suit the conditions of its environment, forms one of the groundworks of the chemistry of to-day.

In examining the progress of Science, we see that she is not afraid to retrace her steps, and that she is able to retain and develop all that is probably true, whilst rejecting all that is proved to be false; and when we learn that she does this, can we hesitate to find in her history the 'promise and potency' of a mighty future?

CAIUS COLLEGE, CAMBRIDGE;
February, 1880.

THE CLASSIFICATION OF THE TERTIARY DEPOSITS.

By Prof. JOHN W. JUDD, F.R.S., Sec. G.S., &c.

THERE are probably few achievements of genius which will bear comparison with William Smith's famous discovery of the true order of succession among the British stratified rocks. Whether we regard the state of knowledge upon the subject at the time when he commenced his labours, or consider how few are the changes which have been introduced into his scheme of classification by the work of subsequent observers, we shall be equally convinced of the justice of his claim to rank as 'the Father of English Geology.'

In his original table, drawn up in 1799, William Smith divided the British strata into twenty-three groups; the highest being the Chalk and the lowest the Coal. And though in subsequent tables he introduced both older and younger formations than these, it is evident that the series of deposits of which the order of succession was perfectly made out by him, embraced only the strata from the Carboniferous to the Cretaceous rocks inclusive. Thus, in 1815, Smith grouped all beds below the Mountain Limestone as Red Rhab, Dunstone, Killas, and Slate, while those above the Upper Chalk are classed in upward succession, as 1, Sand; 2, Crag; 3, Sand; and 4, London Clay.

It remained, therefore, for William Smith's successors to complete his work, by classifying, on the principles which he had laid down, the strata which are older than the Carboniferous and those which are younger than the Cretaceous. With respect to the former, the labours of Sedgwick, Murchison, and Lonsdale, left little to be desired, and the result of those labours was the establishment of the Cambrian, Silurian, and Devonian systems. The distinguished founders of the Palæozoic classification followed William Smith's method of determining the order of succession among strata, which con-

sisted in tracing the beds, as far as possible, by their outcrops at the surface, and determining their superposition in sections, falling back where these methods failed them on 'the identification of the strata by their organic remains.' In the system of nomenclature which they adopted, however, the discoverers of the Palæozoic succession introduced a noteworthy improvement on the method of their great teacher. Instead of adopting names based on the accidental characters of the strata, such as 'Lias,' 'Cornbrash,' and 'Gault,' they employed local names, calling each larger or smaller group of beds after the place in which it was found most typically developed, as the 'Aymestry Limestone,' the 'Bala Formation,' and the 'Devonian System.' This method has been found so convenient in practice, that similar local names have since been given by geologists to many of the divisions of the Secondary strata which had been established by William Smith.

It may, at first sight, appear that in classifying the strata above the Chalk, geologists ought to have followed the same methods and adopted the same principles of notation as had proved so successful in dealing with the older deposits; and it must be admitted that such a course would have produced a uniformity and simplicity in our scheme of geological nomenclature which does not now exist. But a little consideration will convince us that these principles of classification and nomenclature were not abandoned in the case of the Tertiary strata without good cause.

The Tertiary deposits differ from those of Secondary and Palæozoic age in several very important particulars. While the older strata are always of marine and estuarine origin, the Tertiaries include lacustrine and terrestrial beds, often of great thickness. The cause of this difference is found in the fact that, while in the case of the Tertiary deposits, the sea-beds have, perhaps, only once been elevated into dry land since the beds were accumulated, in the case of the older formations they have been subjected to many successive subsidences and elevations and from the denudation which took place during these movements only the deeper-water and more widely-spread sediments have been able to escape. It has been well remarked by Mr. Darwin that 'nearly all our ancient formations, which are throughout the greater part of their thickness *rich in fossils*, have been formed during subsidence;' and it is well known to all geologists that as we go backwards in the geological series we soon find that nearly all traces of terrestrial accumulations disappear; we next lose all relics of fluviatile and lacustrine deposits; then we find littoral marine deposits becoming rarer and rarer; till at last, among the most ancient strata, we seldom find any but such as must have been laid down in deep water,

and are consequently spread over wide areas. Now while the methods of William Smith proved so successful when applied to the uniform and widely-spread deposits of older date, it was soon found exceedingly difficult, if not impossible, to trace the order of superposition, and to represent on maps the outcrops of the inconstant patches of sand, clay, and shell-banks, which often make up the Tertiary formations.

It is to the late Sir Charles Lyell that geologists are indebted for the suggestion of a method whereby the local and inconstant patches of Tertiary strata, which are scattered all over Europe, might be brought into comparison with one another and arranged in groups according to their relative ages. The discovery of this method of classifying the Tertiary strata was an achievement only second in importance to William Smith's determination of the order of succession among the Secondary formations, and would, of itself, serve to place Lyell's name in the foremost rank among the founders of the science of Geology; even apart from the claim which he derives from his masterly exposition of the philosophy of the science contained in the immortal *Principles of Geology*.

The study of the Tertiary formations, as of so many other branches of Geology, was commenced in Italy After the great discussions as to the true nature of fossils, which occupied the minds of the thinkers of that country during the seventeenth century, had been brought to an end by the general acceptance of the doctrine that fossils are not accidental simulacra, but actual relics of once living beings, many able naturalists turned their attention to the study of the rich series of fossils contained in the Subapennine strata. Soldani, Testa, Fortis, Cortesi, Spallanzani, and other Italian naturalists, all recognized the important fact, that while some of the fossils found in the Subapennine strata are referable to forms still living in the Mediterranean, others are now only found in tropical seas, while others again have never been discovered among recent forms and are presumably extinct.

At the commencement of the present century, the illustrious Italian geologist, Brocchi, had made a very extensive collection of the Subapennine fossils, and by a comparison of these with recent shells, he had arrived at the conclusion that more than one half of the Subapennine forms are still living in the Mediterranean, or in other seas, chiefly those of hotter climates. The labours of Brocchi were supplemented by those of Bonelli, Guidotti, and Costa, who added greatly to our knowledge of the several Subapennine faunas.

While this important work was being carried on in Italy, naturalists were not idle in other parts of Europe. Cuvier and Brongniart described the succession of strata in the Paris Basin,

and Lamarck discussed the characters of the invertebrate fauna found therein. In a catalogue of five hundred species of shells found in the Paris Basin, Lamarck showed that only twenty could be identified with living species.

In England, Brander, as early as the year 1766, had described the beautiful fossil shells found at Barton Cliff, in Hampshire, though without discussing their relation to living forms. In 1811 Parkinson described the Crag beds of Suffolk as overlying the London Clay, and as containing numerous fossil shells, of which a considerable proportion could be identified with species now inhabiting the neighbouring seas. About the same time Webster was engaged in studying the Tertiary strata of the Isle of Wight and Hampshire, and in 1813 he established the general parallelism of these beds with those described by Cuvier and Brongniart in the Paris Basin.

In 1820 M. Constant Prévost described the strata of the Vienna Basin, the study of which had occupied his attention during four years, and announced, as his conclusion concerning their age, that they are either younger than the beds of the Paris Basin or equivalent to the upper portion of them.

In the year 1825, De Basterot published an account of a large Tertiary deposit which he had discovered in the basin of the Gironde and the district of the Landes in the south-west of France, and he described more than three hundred species of shells as occurring there. These, as he proved, differ, for the most part, both from the shells of the Subapennine beds, and from those of the Paris Basin. A little later, M. Desnoyers showed that beds with a similar fauna to that collected by De Basterot about Bordeaux and Dax, are found at Touraine, in the valley of the Loire, while Bonelli recognized the same fauna in the hill of the Superga, near Turin.

At the time when Lyell took up the investigation of the subject, the state of the problem was as follows. Three distinct sets of Tertiary strata had been discovered. *First*, the Subapennine beds of Italy and the Crags of England, in which a majority of the shells were found to belong to living species. *Secondly*, the series of strata of the London, Paris, and Hampshire Basins, in which only a very small minority of the shells belong to living species. *Thirdly*, the strata of the Landes, and the Faluns of Touraine (with which those of the Vienna Basin and of the Superga in Piedmont were identified), in all of which the fossils were in great part distinct alike from those of the Subapennine beds, on the one hand, and from those of the Paris and London strata on the other.

One other very important step had been taken by M. Desnoyers, who showed that in the valley of the Loire the beds of Touraine distinctly overlie others containing the same fossils as

those of the Paris Basin. The Italian geologists had also proved that the beds of the Superga underlie ordinary Subapennine strata. Hence it became evident that the formations of the Faluns of south-western France, of the Superga, and of the Vienna Basin, are of younger age than those of the Paris, London, and Hampshire Basin, and of older date than those of the Subapennine hills of Italy and the Crags of England.

It was Lyell who first recognized the remarkable significance which attaches to the fact that the faunas of these series of strata are found to contain gradually increasing numbers of living species as we pass upwards in the series. Brocchi had remarked upon the different results arrived at by Lamarck and himself in comparing the Paris and Subapennine shells respectively with those which are found living in the seas of the present day. But he suggested that the fact might be explained by the difference which undoubtedly exists between the testacea of the Mediterranean and those of the Atlantic Ocean. In 1831, when he completed the second volume of the *Principles of Geology*, Lyell had carefully considered the question of the gradual extinction of old forms and their replacement by new ones, although, as is well known, he felt himself unable to accept any of the theories, which, up to that time, had been suggested for the explanation of the latter class of facts.

All who had the good fortune to know Lyell will recognise it as eminently characteristic of his earnest and truth-loving nature that he delayed the completion of his *Principles of Geology* until he had been able to investigate personally the points at issue, so as to make his descriptions and reasonings as clear as was possible under the circumstances. In the early part of 1828 he had already begun to feel the difficulties which beset the classification of the Tertiary deposits, and he spent the summer in examining the various districts of France and Northern Italy; and in the autumn and winter of the same we find him busy with the corresponding strata in the south of Italy and in Sicily. In 1829, on his way back to England, he revisited Piedmont and some of the French localities, and the summer of that year was devoted to the study of the English Crag beds. In the summer of 1830 the Tertiary deposits of Catalonia, the Pyrenees, and the south of France, were visited by Lyell; and on his return in the autumn, six weeks were spent in studying the great collections of Tertiary fossils brought together by M. Deshayes in Paris.

During these journeys and studies Lyell became convinced that the determination of the proportion of living forms in the marine testaceous fauna of any deposit would enable us to refer it approximately to its position in the geological series; and in

attempting to put this conclusion to the test, he availed himself of the valuable assistance which so able a conchologist as M. Deshayes was able to afford him. Lyell was so impressed by the distinctness of the Sicilian strata, which contain scarcely any but living forms, from those of the Subapennine hills, in which only about one half of the species can be identified with living ones, that he had already determined, before discussing the matter with Deshayes, to divide the Tertiary strata into *four* groups. M. Deshayes had, however, classed his shells in conformity with the facts which had been already made out by geologists into the *three* groups which we have already indicated.

In the choice of names for the great divisions of the Tertiary series, which he was thus led to establish, Lyell determined to indicate the methods by which his results had been arrived at. After consultation with Dr. Whewell, he proposed to call the oldest strata, in which Deshayes' tables showed only 42 forms out of 1238 to be still living (a proportion of three and a half per cent), the Eocene (ἠως, dawn, and καινος, recent), as the beds may be regarded as exhibiting the dawn of the existing fauna of our seas. In the second group of Tertiary strata Deshayes' tables showed 1021 species, of which 176 only were recent forms; while in the third group it was found that more than one half the forms could be identified as still living; hence Lyell called the second group Miocene (μειων, minor, and καινος, recent), as possessing a minority of living forms, and the third, the Pliocene (πλειων, major, and καινος, recent), as yielding a majority of living species. In his original classification Lyell was contented to employ these three terms only, dividing the Pliocene into Older and Newer. In 1839 he erected the Newer Pliocene into a distinct system, in accordance with his original views, giving it the name of Pleistocene (πλειστος, most, and καινος, recent); but as Edward Forbes and other writers employed this term in a sense quite distinct from that proposed by its author, and as synonymous with Post-Pliocene, its use, as indicating a division of the Tertiaries, was formally abandoned by Lyell.

This nomenclature of the Tertiary strata which was proposed by Lyell has been frequently criticized, and is certainly open to the objection that it differs in its principles from that employed in the case of the older rocks. Hence terms have been proposed by various authors as synonymous with Lyell's names, which, like the names of the older rocks, indicate their characteristic features or are derived from the localities in which they are best developed. Thus some geologists prefer to call the Eocene the Nummulitic system, the Miocene the Falunian system, and the Pliocene the Subapennine system. But, on the other

hand, it must be admitted that the Lyellian nomenclature has now acquired such general, and indeed almost universal, currency, that a change of names, which has no other object than the attainment of an impossible uniformity, is greatly to be deprecated And there is, moreover, much to be said in favour of Lyell's names, not only on the ground of priority, but from the fact that they indicate the principles on which Lyell's classification is based, and serve as a monument—the best possible monument, indeed—of the great advance in systematic geology which resulted from his labours.

The value of the Lyellian method of classification is illustrated by the fact that in the third volume of the *Principles* its author was able to bring into more or less exact correlation the numerous and widely scattered Tertiary deposits which had been detected in various parts of the Continent. Since 1833, when this work appeared, Lyell's broad general outlines of the succession of events during the Tertiary epoch have been filled up in great detail by the labours of Prestwich, Constant Prévost, Hébert, Dumont, Sandberger, Beyrich, Hornes, Fuchs, Heer, Carl Meyer, and other investigators; and in the application of Lyell's method, its capabilities, and also its imperfections and shortcomings, have been brought to light by the test of practical experience. It will be instructive to notice the respects in which the Lyellian method, after being tried, has been found wanting.

First among these, we may notice the circumstance that, as the acquaintance of geologists with the Tertiary formations has become more and more extended, deposits have come under their notice which it has been found difficult to refer to either of Lyell's divisions, and which prove to constitute transitional formations linking two of them together. Thus the geologists of South Germany have shown that in the Vienna Basin the Miocene strata graduate so imperceptibly into the Pliocene, that they have been led to unite these two divisions, and call them the 'Neogene.' But this class of objection cannot be regarded as especially applying to Lyell's classification, for it is certain, from the very nature of the case, that whatever grouping of strata we may choose to adopt, certain deposits must be found which refuse to accommodate themselves to the artificial system.

A more serious objection to Lyell's classification is based on the fact that at the period when he devised it, geologists were not acquainted with all the great representative faunas of the Tertiary epoch, and that since his time very important deposits have been discovered which cannot be referred to either of Lyell's groups. The great brown-coal deposits of Northern Germany, which are now known to be of the age of the English Barton Clay, the highest member of the Eocene, are reached

by shafts, which penetrate a great thickness of drift-sand, gravel, and clay. Immediately overlying the brown-coal, beds containing marine shells were found at many points by the German geologists; and these fossils, when carefully studied, were found to differ so greatly from those of the Eocene on the one hand, and the Miocene on the other, that it was difficult to refer the beds containing them to either of Lyell's divisions.

At the same time, the researches of Nyst and Dumont in Belgium, and of Sandberger in the Mayence Basin, made geologists acquainted with other deposits containing a fauna similar to that found in the North German beds, and, like it, distinct from the faunas of both the Miocene and the Eocene.

Somewhat later, the execution of certain new railway works near Paris enabled MM. Hébert and Raulin to make a careful study of the fauna of the Fontainebleau Sands, which had hitherto remained almost unknown. And in these beds were found assemblages of fossils agreeing very closely with those of the North German and Belgian localities, but differing, like them, from the faunas both of the Eocene and Miocene deposits.

Lastly, the construction of a branch of the South-Western Railway through the New Forest in Hampshire led to the exposure, in a railway-cutting near Brockenhurst, of marine beds containing many mollusca and corals, which were collected with untiring industry by the late Mr. F. Edwards. Similar fossils had previously been found at Lyndhurst and at some other points, both in the New Forest and in the Isle of Wight, but those obtained in the Brockenhurst railway-cutting were so numerous and well preserved as to awaken general attention and interest in the subject. An examination of the Brockenhurst mollusca by Herr von Koenen proved conclusively that they agree most closely with the fossils of the North-German clays and sands, with those of the Fontainebleau Sands, and with those of the equivalent strata in Belgium and the Mayence Basin. Dr. Duncan's examination of the Brockenhurst corals led him to precisely the same conclusion, which was arrived at by him quite independently of Von Koenen's researches.

It thus came to be a recognized fact that in North Germany, in the Mayence Basin, in Belgium, and in the Paris and Hampshire Basins, there exist strata which at some points are seen to be superimposed on Eocene strata, and at others are found underlying the Miocene, and that the fauna of these beds is very distinct both from that of the Eocene and from that of the Miocene.

In the year 1854 Professor Beyrich proposed that to this new geological horizon, which had come to be recognized as existing between the Eocene and the Miocene, the name of Oligocene (from ὀλιγος, few, καινος, recent) should be given, the

name being formed on the same principle as was adopted by Lyell in coining his terms. Exception has been taken to this name of Professor Beyrich's, but its very general adoption may be taken to prove that it supplied a want which was beginning to be very generally felt. It is true that Lyell did not adopt this new term, but it may be safely asserted that, had he lived to witness the recent advances in our knowledge of the deposits of this period, he would not have hesitated to accept the evidences in favour of its employment. Not only in Germany, Belgium, France, and England, but in the Alps and Eastern Europe, have beds of great thickness and importance, which must be referred to this division of the geological series, been discovered, and even in North America it has been shown that it is convenient to employ the term Oligocene to designate a division of the Tertiary series.

It will be interesting to take a brief survey of the great series of deposits which recent researches have led geologists to refer to the Oligocene period; and in doing so we shall notice the evidence which they afford of the distribution of the land and sea, and the general physical features of Europe at the time of their deposition.

It is in Eastern Europe that the Oligocene strata acquire their greatest normal thickness and development. In Transylvania and Hungary marine and brackish-water strata of this age are found, attaining a thickness of betwen 2000 and 3000 feet. Dr. Anton Koch, of Klausenberg, has described the succession of the Oligocene strata in the neighbourhood of that town, where, resting upon beds containing the fauna of the Barton Clay, we find a thick series of deposits which can be referred to the Upper, Middle, and Lower Oligocene respectively. I have myself had the opportunity of studying the succession of those Transylvanian beds, and of noticing how the series of fossiliferous beds in that area can be paralleled with those of northern and western Europe. A similar succession has been made out in the Bihar Mountains and in other parts of Transylvania. As we proceed southward, however, towards the Turkish frontier, we find brackish-water and terrestrial conditions prevailing over the marine. This is well illustrated in the Zsil Valley, in the south of Transylvania, where at Petroseny a coal-bed, nearly ninety feet in thickness, is being worked in open pits and by means of adits. This great coal-seam and others of lesser thickness are intercalated in a series of strata, which contain a characteristic Oligocene fauna, the strata in question having been preserved from denudation through being thrown into a synclinal fold.

In the country round Buda-Pest the Oligocene strata have been carefully studied by Dr. Szabó, and their fossils have been

described by Dr. Von Hantken. A deep well-boring near the city has fully confirmed the conclusions which had been arrived at as to the great thickness (more than 2000 feet) of the marine Oligocene strata in this area. Underlying the great masses of Miocene lava that cover so large a portion of Hungary there are found at many points marine strata, which by their fossils must be referred to the Oligocene. These beds, as developed near Vissegrad, have been well described by Dr. Koch.

When we proceed westward, into the Vienna Basin, we find that the Middle and Lower Oligocene strata are altogether wanting in that area, and only the Upper Oligocene is represented by the 'Aquitanische Stufe.' In passing southwards, into Styria, Croatia, and Sclavonia, these Upper Oligocene strata are found to consist of brackish-water and terrestrial deposits, just as is the case farther west in Transylvania.

In Northern Germany, throughout the whole of the drift-covered districts, stretching from Warsaw to Hamburg, patches of Oligocene strata are here and there found rising above the superficial covering of the country, and the same deposits are constantly met with in wells and borings. These strata contain marine fossils, sometimes of littoral, at others of a deeper-water character. It is in these beds that the German geologists have discovered such a rich molluscan and coral fauna. The most abundant stores of fossils have been obtained during the sinking of pits to the brown-coal beds (which are of the age of the Barton Clay), the shafts often passing through richly fossiliferous clays and sands before reaching the lignite beds.

As is the case in Hungary with the Oligocene strata, so we find with the marine beds of that age in Northern Germany that when traced southwards they graduate into estuarine, fresh-water, and terrestrial formations. Opening into the great North German Oligocene sea we find clear evidences of the existence of four or five great deltas at this period. The most easterly of these now forms the country of Lower Silesia; next we have the delta of the Saxon district; thirdly, we find the delta of the Lower Rhine, of which the Mayence Basin may be regarded as a part; and fourthly, the estuarine deposits of Oligocene age in the Netherlands, and in the Paris, and the Hampshire Basins, which are all very closely connected with one another, and may have formed parts of several more or less united deltas. In the North German deltas we find beds of brown coal (distinct from and of younger date than the great Upper Eocene brown-coal formation), which alternate with sands and clays containing fresh-water or brackish-water fossils. Farther westward, we have only thin lignite deposits, the strata consisting of fresh-water limestone, clays and sands, with occasional marine beds intercalated among them.

Now while these marine and estuarine strata were being accumulated in Hungary, Northern Germany, the Netherlands, Northern France, and the British Islands, a number of great lakes existed upon the land of the Oligocene period. These were gradually filled up, and obliterated by sedimentary deposits. Owing to the fortunate circumstance that, immediately after the close of the Oligocene period, a grand outburst of the volcanic forces took place over very wide areas, these fresh-water deposits have been covered up and protected to some extent from destruction by denudation. It is thus that we find preserved for our study the interesting lacustrine deposits of the Limagne, of Montbrison, of the Haute Loire, of Menat, and of many smaller lakes in the Auvergne, and of the lake-basins of Teplitz, Falkenau, and Eger, in Bohemia. All of these lakes, with many others, of which all traces must have been removed by denudation, seem to have been in existence during the Oligocene period.

If we now turn our attention to the districts immediately adjoining the great Alpine chains, we find the Oligocene formation represented by masses of strata of enormous thickness; clays, sands, and conglomerates, accumulated sometimes in fresh-water lakes, at others in gulfs connected with the ocean. The Oligocene deposits of the Alpine district are estimated by the Swiss geologists as attaining a thickness of no less than from 10,000 to 12,000 feet, and present a marked contrast in their physical character to the marine, estuarine, and lacustrine beds of the same age in other parts of Europe. But when we come to study the fossils of these Alpine formations, the parallelism of their several members with the divisions of the Upper, and Middle, and Lower Oligocene of Northern Europe becomes strikingly apparent. In the adjoining table an endeavour has been made to illustrate the correlation of the Oligocene and underlying deposits as exhibited in different parts of Western Europe (see p. 133).

It is evident that the great and general subsidence which took place in the European area during the Eocene or Nummulitic period had already come to a close, and had been succeeded by general elevatory movements before the commencement of the Oligocene. A great part of what is now Central Europe had become dry land, while an open sea stretched to the north and north-east of it. The Oligocene deposits were accumulated along the shores, and in the deeper waters of this sea, and in the estuaries of the great rivers which flowed into it from the south. At the same time great numbers of lakes existed on the surface of this Oligocene land, and into these were washed and thus preserved for our study many of the land animals and plants of the period. Before the close of

CORRELATION OF THE LOWER TERTIARY STRATA OF WESTERN EUROPE.

		London Basin.	Hampshire Basin.	Paris Basin.	Netherlands.	Northern Germany.	Switzerland.
Oligocene.	Upper.			Freshwater Limestone of Beauce.	Beds of Maestricht.	Fossiliferous Strata of Osnabrück, Cassel, &c. Brown Coal of Rhenish Prussia.	Aquitanian Stage and Lower Brown-coal Formation.
	Middle.		Hempstead Series. Bembridge Group.	Fontainebleau Sandstone.	'Rupélien.' 'Tongrien Superior.' (Kleyn Spawen Beds.)	Septarian Clay. Marine Sand of Mayence Basin. Brown-coal Formation.	Upper Marine Molasse (Tongrian).
	Lower.		Brockenhurst Series. Headon Group.	Gypsum of Montmartre. Cerithium concavum. Beds of Mortefontaine, &c.	'Tongrien Inferior.'	Fossiliferous Beds of Egeln, Magdeburg, &c.	Lower Marine Molásse (Ligurian).
Eocene.	Upper.	Bagshot Series.	Barton Clay. Bracklesham Series and Bournemouth Beds.	Sands of Beauchamp. 'Calcaire Grossier.'	'Laekenien.' 'Bruxellien.'	Great Brown-coal Formation.	Nummulitic Strata and Flysch.
	Middle.	London Clay.	Bognor Beds.	Sands of Soissons.	'Ypresien.'	...	Lower Eocene Sandstone.
	Lower.	Oldhaven Beds. Woolwich and Reading Beds. Thanet Sand.	Plastic Clay.	Plastic Clay. Strata of Meudon, Rilly, and Nemours.	'Landénien.' 'Heersien.' Limestone of Mons.		

the Oligocene period we have proofs of the commencement of that series of volcanic outbursts which attained their climax in the succeeding period of the Miocene. But along the great Alpine axis movements on a grander scale were taking place, which resulted in the formation of vast lakes, in which great thicknesses of strata were accumulated; these lakes being sometimes, by the action of the subterranean forces, placed in communication with the open ocean. At the end of the period the elevatory forces so far prevailed over those producing subsidence, that the whole area was converted into a great continent, which remained above water during the Miocene but was to some extent submerged in the Pliocene and re-elevated in the recent period. Such appears to be the succession of changes in the physical geography of this part of the Earth's surface during the several Tertiary epochs.

It is a very interesting circumstance, as I have pointed out in a memoir recently laid before the Geological Society of London, that we have in the Hampshire Basin very beautiful and interesting representatives of at least the Middle and Lower divisions of the Oligocene system. Owing to an unfortunate error in determining the order of succession of these beds, their thickness has been hitherto greatly under-estimated, and they have been grouped with the Eocene by some authors and divided between the Eocene and the Miocene systems by others. No fact, however, can be more certain than that those fluvio-marine strata of the Isle of Wight and the New Forest are the representatives of the great Oligocene system of the Continent. The new classification which is now proposed for them is as follows:—

Upper Oligocene.	Wanting in the British Islands.
Middle Oligocene.	Hempstead Series (marine and estuarine), 100 feet. Bembridge Group (estuarine), 300 feet.
Lower Oligocene.	Brockenhurst Series (marine), 25 to 100 feet. Headon Group (estuarine), 400 feet.

Whether we study the marine mollusca, the fresh-water and terrestrial testacea, the reptilian and mammalian fauna, or the terrestrial flora of the period, we find the most convincing proofs that these strata of the Hampshire Basin are the exact equivalents of that great system of strata which has received the name of the Oligocene upon the Continent, which, as we have seen, attains to such enormous thickness and importance in some areas, and which everywhere is so well characterized by the distinct assemblage of fossils which it contains.

It is a remarkable circumstance that nearly all the great

capitals of Europe—London, Paris, Brussels, Vienna, Berlin, among the number—stand upon deposits of Tertiary age. This circumstance has doubtless powerfully contributed to the great amount of attention which has been devoted to the study of the order of succession, and the collection and comparison of the fossils of these deposits. We have already referred to the causes that prevented the earlier recognition of the importance of the Oligocene deposits, which occur but are not well exposed in the immediate vicinity of the North German capital. The Hampshire Basin has not the distinction of constituting the site of a great city, and its beds have not been so diligently explored as have those of the London and Paris Basins. The labours of Webster, Prestwich, and Forbes have, however, done much towards making clear the order of succession of the strata of the Hampshire Basin. Others, like the late Mr. Frederick Edwards and Mr. Searles Wood, have devoted themselves to the collection of the fossils of the district. It is to be hoped that the valuable fossils from the Oligocene strata of the Hampshire Basin which are contained in the British, the Woodwardian, and other Museums, a very large proportion of which still remain undescribed, may before long be made generally known to science by means of description and figures; for it is certain that a more accurate acquaintance with these Hampshire strata and their fossils will enable geologists to make valuable improvements in the classification and correlation of the lower and middle Tertiary deposits in various parts of Europe.

ARTIFICIAL DIAMONDS.

By F. W. RUDLER, F.G.S.

WHY does Science smile approvingly on the modern chemist in his efforts to produce the diamond, and yet frown upon the old alchemical notion of producing gold? If the one substance can be prepared by art, why not the other?

Everyone knows that these two bodies are the most highly valued of all natural products, and for that reason it was long suspected that some occult kinship must of necessity exist between them. Thus Pliny, speaking of the diamond, says, 'It seemeth that it should grow nowhere but in gold.' Much as the ancients prized gold, they prized this gem—the invincible *adamas*—still more. The earliest mention of the true diamond, according to the Rev. C. W. King, is by the poet Manilius, who describes it as *pretiosior auro*. 'The Diamant,' says Pliny, to quote Dr. Holland's quaint translation, 'carrieth the greatest price, not only among precious stones, but also above all things else in the world: neither was it knowne for a long time what a Diamant was, unlesse it were by some kings and princes, and those but very few.' But since those early days science has grown wondrously familiar with the diamond, and has even been bold enough to attempt its fabrication. The chemist has, in fact, outrun the alchemist: the one sought merely to make the precious metal, but the other seeks to make the yet more precious gem. Nevertheless, we treat the alchemist with ridicule, while we watch the diamond-making chemist with the keenest interest!

The truth is, that the value of the diamond, unlike the value of the gold, lies not in the *matter* of which it is composed, but only in the peculiar *form* in which that matter exists. In attempting the preparation of a diamond, we are not, therefore, striving after the impossible; we are not seeking either to create matter or to transmute one elementary species of matter

into another; all that we attempt is, to bring the given kind of matter into such a physical condition that it shall possess the set of properties which we so highly prize in the diamond.

About a century ago the chemical composition of the diamond was first carefully determined, and a fresh light was then cast upon the gem. From the day when it was ascertained that the diamond consisted only of carbon, its artificial preparation came within the range of possibility. The old notions of its kinship were entirely changed, and it was unexpectedly found that such vulgar substances as blacklead and charcoal could claim close relationship with the costly gem. Pliny ridicules the idea that the diamond could be found, as Metrodorus Scepsius had affirmed, in a locality 'wherein amber is engendered;' and the old philosopher does not hesitate to say of this authority, 'howbeit no man doubteth that he lieth stoutly.' But, after all, this notion of the relation of the diamond with amber is more sound, from a chemical point of view, than Pliny's own notion that diamonds 'breed not but in mines of gold.'

Knowing the chemical composition of the diamond, the mystery of its formation resolves itself into this problem: How to crystallize a given piece of carbon in the special forms which the gem possesses, and with the accompanying transparency, lustre, and hardness? Difficult as the solution may seem, men of science have long believed it to be practicable. 'We are so sanguine about this matter,' said Dr. Percy, when lecturing on chemical geology in 1864, 'that we cannot refrain from believing that one day or other the thing must be done. *It assuredly will be done.* We have apparently been very near it from time to time, but have never yet reached it.' These prophetic utterances have recently received a most unexpected fulfilment, which it is the purpose of this article to chronicle.

About three months ago, Mr. James Mactear, of the St. Rollox Chemical Works at Glasgow, created considerable excitement by announcing that he had succeeded in producing a crystallized form of carbon, comparable, if not identical, with diamond. It is acknowledged that this gentleman brought extensive chemical knowledge to bear upon the subject, and that he struck out a most promising line of research. Nevertheless, his announcement was confessedly premature; and it remains doubtful whether anything that can fairly be called diamond was ever produced in his researches. At any rate, the small crystalline particles which were at first taken to be diamonds, gave a most unsatisfactory account of themselves when subjected to Professor Maskelyne's searching examination,

and they utterly collapsed under the chemical scrutiny of Dr. Flight.

It has been well said with reference to other subjects that 'the failures of the past prepare for the triumphs of the future.' Nor is this saying inapplicable to our would-be diamond manufacturers. Scarcely had Mr. Mactear's investigations faded from the public mind, when Mr. A. H. Allen, of Sheffield, put in a claim on behalf of Dr. R. S. Marsden; and before this second process is revealed, Mr. J. Ballantine Hannay, a young Glasgow chemist, steps forward and actually places in our hand an artificial diamond!

For some time past Mr. Hannay has been engaged in a most interesting series of researches which have unexpectedly led up to the present discovery. To appreciate these researches it is necessary to turn to a subject which appears, at first sight, to have no bearing whatever upon the artificial production of the diamond.

More than half a century ago, Cagniard de la Tour made some remarkable experiments to determine the effect of heat upon liquids closely sealed in strong tubes. This inquiry was afterwards followed up by Dr. Andrews, of Belfast. He showed, for example, that carbonic acid gas above a certain temperature cannot be liquefied by means of pressure; but the gas, if compressed, assumes a condition which is neither that of a liquid nor that of a gas. Let the temperature be lowered, and it becomes a true liquid. Let the pressure be lowered, and it becomes a true gas. It was found that the two physical states of liquidity and gaseity pass by insensible transition one into the other; the continuity between the two conditions being perfect. That particular temperature, above which pressure does not produce liquefaction, is termed the *critical point*.

Reverting to the experiments of Cagniard de la Tour and Andrews, in which liquids were heated in closed tubes, let us suppose a solid to be dissolved in the liquid, and the solution to be then raised beyond its critical point. What will occur? The liquid will pass into the gaseous condition; but what will become of the solid? This is the question which Mr. Hannay, working in conjunction with Mr. Hogarth, sought to answer. At first sight it might be fairly assumed that if the solid were not volatile at the temperature to which it was exposed, it would be incapable of assuming the gaseous condition, and that it would therefore be abandoned by the solvent: hence, when the menstruum passed through the critical state, and became gaseous, the dissolved body would be precipitated in a solid form.

Such an assumption, however, was flatly contradicted by experiment. It was soon found that in many cases the solid

body was *not* deposited, but remained in a state of solution or diffusion in the gas. We are thus brought in contact with the unexpected phenomenon of a solid substance being *dissolved by a gas*, just as it might under ordinary circumstances be dissolved by a liquid.

Since water is the most generally useful solvent, it might be supposed that such experiments would be best made with aqueous solutions. Practically, however, the use of water is precluded, on account partly of its inconveniently high critical point, and partly of the fact that water at a high temperature and under great pressure is capable of exerting a corrosive action upon the glass tubes in which the experiments are undertaken. A more convenient solvent was found in alcohol, and many of the early experiments of Messrs. Hannay and Hogarth were made with a solution of iodide of potassium in this menstruum. A strong tube was about half filled with an alcoholic solution of potassic iodide; the extremity was sealed, the tube placed in an air-bath and heat applied. Having passed through the critical stage, the alcohol became gaseous; but the iodide, instead of being precipitated, remained in solution in this gas. Even when the temperature rose to 380° C., or about 150° above the critical point, the alcohol-gas still asserted its solvent power over the solid salt. Moreover, by an ingenious arrangement, it became possible to expose a fragment of the iodide to the action of the gas without allowing it ever to come in contact with the liquid; yet the solid slowly disappeared, and was at length completely dissolved by the invisible solvent. But on rapidly releasing the gaseous solution from the pressure to which it had been exposed, the iodide was precipitated, either as a cloud of delicate snow-like crystals, or as a crystalline film, like hoar-frost, on the inside of the glass tube. On again increasing the pressure, however, the crystals were re-dissolved, and once more disappeared.

Here then a new light broke in upon the phenomenon of solution. Hitherto it had been supposed that only liquids possessed solvent powers, but Messrs. Hannay and Hogarth have now shown that gases also are similarly endowed. In short, these researches fortify the conclusion which Dr. Andrews had previously reached, that there is perfect continuity between the liquid and the gaseous conditions.

If such extraordinary solutions can be effected, what more natural than to inquire whether *carbon* could be caused to dissolve in some appropriate solvent? Carbon is a remarkably obstinate body, resisting all ordinary menstrua, such as acids and alkalies, alcohol and ether. It is worth noting, however, that molten cast-iron can dissolve carbon; and that when the metal cools the carbon is partially separated in crystalline

scales, resembling graphite. Such scales are known to workmen under the curious name of *kish*.

Every schoolboy knows now-a-days that carbon occurs in nature crystallized as two entirely distinct minerals: in the one form it is known as *graphite, plumbago*, or *black lead*; in the other form as *diamond*. Metallurgists, as just stated, are familiar with the artificial production of graphite, and this body has also been produced by certain chemical reactions; but the artificial crystallization of carbon in the form of diamond has heretofore invariably baffled the chemist.

While the air of Glasgow was filled with the rumours of Mr. Mactear's experiments, it was natural to turn to Messrs. Hannay and Hogarth's researches, if haply their new method of gaseous solution might lead us to the desired end. They found that when a solid is freed from its gaseous solvent, it is invariably deposited in a crystalline condition. Now, if carbon could be thus dissolved, there was, of course, the bare possibility that it might be deposited in the crystalline form of diamond.

On applying himself to this inviting problem, Mr. Hannay was disappointed to find that all the forms of carbon with which he experimented, such as graphite, or charcoal, or lamp-black, obstinately refused to yield to any of the solvents which he brought to the attack. It was clear, therefore, that if the problem was to be solved at all it must be solved in an indirect manner, and Mr. Hannay's ingenuity was equal to the occasion.

Carbon is remarkable for the multitude of volatile compounds which it is capable of forming with hydrogen. Now Mr. Hannay found that when a gas containing carbon and hydrogen is subjected to heat under great pressure in the presence of certain metals, such as magnesium or sodium, the hydrocarbon is broken up, and its hydrogen combines with the metal, while its carbon is set free. In order to command the high temperature and the intense pressure necessary for this reaction, Mr. Hannay employs wrought-iron tubes, about 3½ inches in thickness, and yet these are frequently torn open in the course of the experiments.

It appeared probable that the carbon set free in this decomposition might, at the moment of its formation, or when in the *nascent* condition, be dissolved by the gas, and then, on a reduction of pressure, be precipitated in a crystalline condition. Mr. Hannay has found that in order to obtain the carbon in the required crystalline state it is necessary that a stable compound containing nitrogen be present. When these conditions were fulfilled, the operator had the satisfaction of

finding that some of the carbon which was set free actually crystallized in the form of diamond!

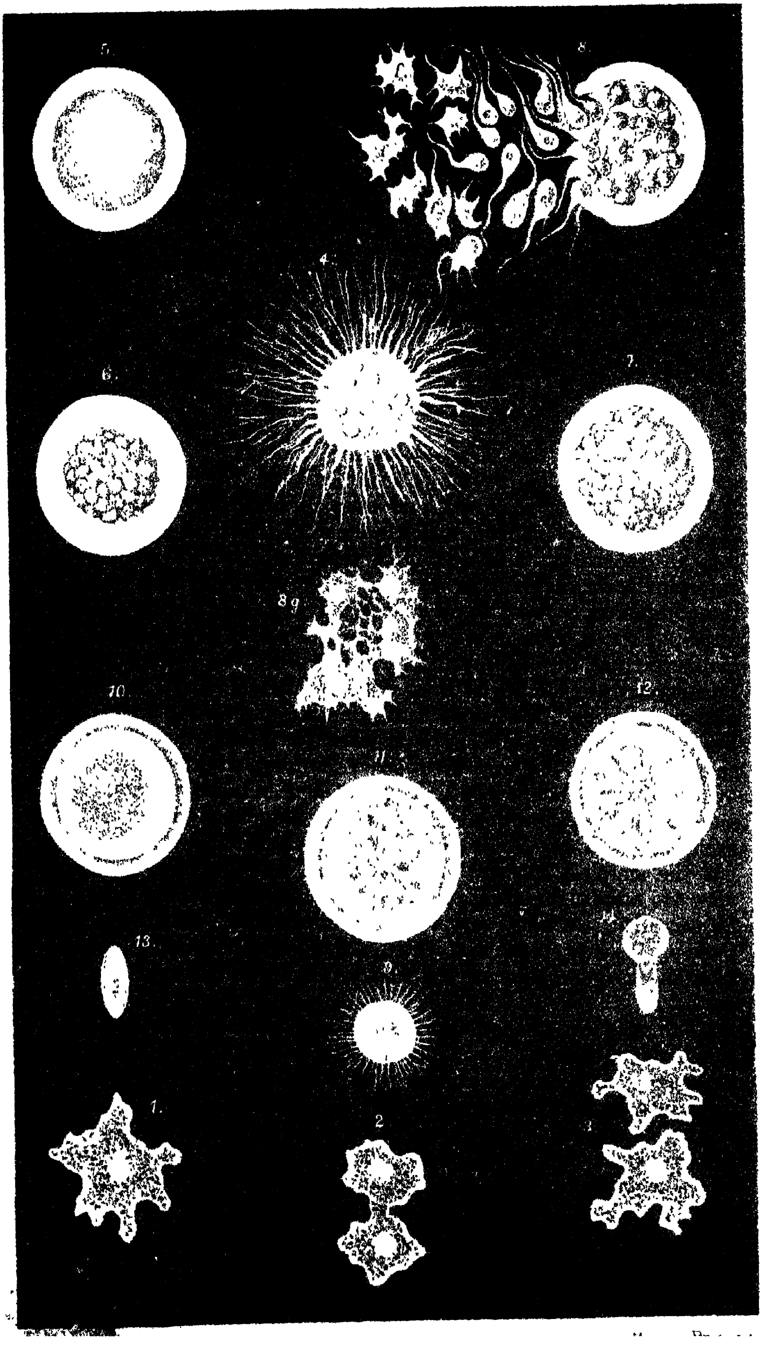
This adamantine carbon has been severely tested, not only by the discoverer himself, but also by so high a mineralogical authority as Professor Maskelyne. First, as to *hardness*, which of all characters is the most characteristic and the most valuable in diamonds: it is found that Mr. Hannay's crystals will easily scratch deep groves in a sapphire, and no substance save diamond possesses this strong abrading power. With regard to *crystalline form*, little can be said, but still that little is quite satisfactory. Perfect crystals have not, as yet, been obtained, and the fragments look like splinters of diamond rather than crystals. Still in one case Professor Maskelyne found traces of the distinctive octahedral cleavage, and Mr. Hannay has called attention to the curvature of some of the faces, so suggestive of diamond-crystals. *Optically* the crystalline fragments behave themselves just as diamonds might be expected to behave. Moreover, when placed in the scales they are not found wanting, for some of the artificial adamantoid carbon has as high a *specific gravity* as 3·5. Finally, the *chemical tests* leave nothing to be desired. Heated in the voltaic arc the carbon swells up and turns black, just as is the case with diamond; while if burnt in the usual way, in oxygen, it yields only carbonic acid; and though but a very small quantity was operated on, the result showed that the artificial crystalline body contained as much as 97·85 per cent of carbon. All the lines of evidence therefore converged to this point, that we are here dealing with a substance which is to all intents and purposes neither more nor less than *diamond*.

It thus appears that Mr. Hannay has mimicked Nature so successfully as to produce a body not distinguishable from the natural gem. In connexion with this interesting discovery, however, two questions naturally suggest themselves: first, has the artificial substance been produced in the same way as the native diamond? And, secondly, can the artificial product be made in such quantity and with such facility as to be profitably sent into the market?

The first question is by no means easily answered. Nature has such a wealth of resource at her command that in compassing a particular end she is by no means limited to a single method. Nothing is more likely than that the diamond has been formed in one way in this locality and in another way in that. In fact, the conditions of its occurrence are so dissimilar in different parts of the world as to make it highly probable that the diamonds of Brazil and the diamonds of South Africa have been brought forth by different processes. Mr. Hannay may or may not have hit upon an exact imitation of the natural

conditions of diamond-making, but even if he has gone to work after one of Nature's ways, it is far from necessary to assume that all diamonds have been fashioned in this particular manner. It was an old alchemical dogma that 'Vulcan is a second nature, imitating concisely what the first takes time and circuit to effect.' Obedient to this maxim, Mr. Hannay has pressed Vulcan into his service, but a good deal that we know about the natural diamond in certain localities tends to show that Vulcan has not always presided at its birth. Thus an eminent chemist recently said, 'We are entirely ignorant of the mode of its formation in Nature. The only thing which may be regarded as certain is that it has *not* been formed at a high temperature.'

After all, the genesis of the diamond is a subject of only scientific interest; the practical question for unscientific folk is whether Mr. Hannay can or cannot make his product in sufficient quantity to disturb the diamond market. Owners of gems, however, may be comforted by the assurance that, at present, the artificial specimens are small in size and costly to procure. When the chemist has completed his elaborate experiment, and opened the iron tube which has just been drawn from the furnace, he finds that his diamonds are not, like Sinbad's, 'of a surprising bigness.' They are, in fact, rather of a surprising smallness. But, were they no bigger than pins' heads, the experiment would still be a memorable scientific triumph. Practically, however, there is all the difference in the world between a laboratory experiment and a manufacturing industry. At the same time, it is of course possible that the recent Glasgow experiments may be merely the grains of mustard-seed which shall be eventually developed into a fruitful undertaking of commercial significance. Nothing, however, is more certain than that Mr. Hannay commenced his researches without the slightest regard to what Bacon calls 'the applying of knowledge to lucre.'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THE THRESHOLD OF EVOLUTION.

BY SURGEON-MAJOR WALLICH, M.D.

[PLATE IV.]

IN order to determine, with at least approximate certainty, the starting-point from which animal life took its earliest step in evolution, it is obviously essential that we should entertain accurate ideas as to the nature and characters of those humblest types of being that stand at the bottom of the series. Fifteen years ago it was the general belief amongst biologists that the Rhizopods, as presenting 'the distinctive attributes of animal life in their least specialized shape,' occupied the position. But, since then, scientific opinion has undergone an important, and, as I venture to think, prejudicial change, through the promulgation by Professor Hæckel of the hypothesis that there exists a third kingdom in creation, intermediate between the animal and vegetable kingdoms, the lowest group of which, *the Monera*, comprises 'not only the simplest, but the simplest conceivable organisms;' and hence, with a view 'to satisfy the requirements of the human understanding for causality,' we must accept as an 'unprovable,' but, nevertheless, absolute fact, that these organisms 'originated by spontaneous generation at the first beginning of life upon the earth.'

The chief aim of the following observations is to prove, on the basis of Hæckel's own descriptions and figures, that the structures in question have no *locus standi* as Monera; and, consequently, that we are as far off as ever from being able to say with confidence that they form the connecting link between the organic and inorganic world; or that they were 'the first molecules of matter that took upon themselves the responsibility of living.' *

* This expression was used satirically by Dr. Dawson, of Eozoon celebrity, in speaking of spontaneous generation.

Had Hæckel's statements concerning the Monera reached their climax when he made the extraordinary announcement that, in spite of each individual particle of which their bodies are composed having been proved, by the most refined chemical and optical tests, to be but an exact counterpart of every other particle entering into their composition—eight genera and sixteen species were readily distinguishable, a statement so tantamount to affirming that things which are equal to the same thing are NOT equal to each other, might safely have been left to find its own level. But when, in addition to this, an effort is made to upset every heretofore proposed classification of the Rhizopoda and Protozoa generally, on such untenable evidence as that upon which the existence of *Bathybius*, *Protamœba*, *Protomyxa*, *Protogenes*, and *Myxastrum*—all typical Monera be it observed—avowedly depends; and we are gravely asked to believe that '*the most remarkable of all Monera*, BATHYBIUS, probably even now always comes into existence by spontaneous generation,' in the depths of the ocean; it will, I think, be freely admitted that the matter demands a much more searching investigation than has hitherto been bestowed upon it.

I shall have occasion, hereafter, to allude to some of the minor evils resulting from this speculative style of teaching. But lest it be imagined that I am exaggerating the facts, I would invite attention to the two subjoined brief passages from the writings of Mr. St. George Mivart, which, though apparently written under a singularly erroneous view of the scope and limits of modern biological inquiry, unmistakably indicate that even amongst those who might reasonably be supposed to know better, 'the doctrines of evolution' and spontaneous generation are regarded as mere extensions of the same order of phenomena—relating, on the one hand, to organic life, and, on the other, to inorganic matter.

Thus, referring specially to Hæckel's observations upon the lowest forms of life, Mr. Mivart expatiates on the materialistic pantheism and the atheistic deductions from supposed facts which later investigations have proved to be fictions, '*e. g.*, the supposed organism *Bathybius Hæckelii*;' and declares that '*the doctrines of evolution logically culminate in three negations—namely, of God, of the soul, and of virtue.*'

Even Buchner, the earnest and undaunted advocate of freedom of thought and teaching, in his treatment of an essentially speculative department of knowledge, recognizes the necessity of rejecting mere hypothetical evidence in dealing with natural science. In his remarkable work, *Kraft und Stoff*,* whilst

* An excellent translation of Buchner's work, entitled *Force and Matter*, edited by Mr. T. F. Collingwood, F.G.S., has gone through two editions since 1864.

expressing the opinion that 'spontaneous generation undoubtedly played a more important part in the primeval period of the world's history than at present,' frankly allows, notwithstanding his unflinching faith in the potentialities of matter, that 'we possess no certainty nor well-founded data on the point, and are ready to confess our ignorance. But, though as regards organic creation much may be doubtful, we may positively assert that it may have, and has, *proceeded* without the interference of external forces.'

Now there is certainly nothing illogical in the opinion as thus stated, nor can it with fairness be looked upon as extreme even by the moderate Evolutionist. It is a simple avowal of the sufficiency of natural causes and conditions to account for the phenomena of life, *when once established*, and an outspoken warning that a rigid line of demarcation should always be drawn between that which admits of proof and that which at best can only be surmised.

Professor Virchow, another of Hæckel's countrymen and his quondam teacher, one of the most accomplished biological writers this century has produced, has yet more emphatically declared that where the object of a speaker or writer is to instruct, 'no hypotheses or assumptions, however dogmatically insisted on, can stand in the place of details, the results of observation, experiment, and thought.' It is, therefore, not surprising that Virchow should have vehemently attacked the Hæckelian system, or that two men with minds so antithetically constituted should thereupon have engaged in a controversy almost without parallel for the acerbity with which it has been conducted.

Let it not be imagined, however, that Hæckel makes any secret of his devotion to purely 'speculative science.' In his reply to Virchow, entitled *Freedom in Science and Teaching* (1879, p. 63), he says: 'In my opinion there is no boundary-line between the speculative departments of science and those that are actually conquered and firmly established; on the contrary, all human knowledge is subjective.' And in support of this proposition he then proceeds to show that even mathematics, which stands at the head of the exact sciences, is based entirely 'on those deepest and simplest fundamental axioms which are incapable of proof. It will be seen presently to what extravagant lengths 'freedom in teaching' may be carried where there is a preconceived hypothesis to support.

According to Hæckel, the Monera stand at the bottom of his list of *Protista*, which are *said* to constitute a very natural assemblage of organisms, presenting characters more or less common to animals and plants, but, in point of fact, comprising the *Foraminifera*, *Amœbæ*, and *Noctilucæ*, which are unquestionably animals; and the *Diatomaceæ*, '*Oscillarineæ*,' and certain

Fungi, which are as unquestionably plants. Whilst the *Gregarinæ* are made a sub-family of the *Amœbæ*, the *Amœbæ* are separated from the *Rhizopoda*; and the *Foraminifera*, *Heliozoa*, and already incongruous *Radiolaria*, are served up as an *olla podrida* under the name of the *Rhizopoda!*

But let us now direct our attention to the special group with which we are more immediately concerned, namely, the Monera.

With a marvellous eye to the value of precise dates, Hæckel tells us that the Monera originated in the beginning of the Laurentian period, 'by spontaneous generation as crystals from the matrix, out of simple inorganic combinations of carbon, oxygen, hydrogen, and nitrogen. If we tried to constitute, *a priori*, the simplest conceivable organisms, we should always be compelled to fall back upon such Monera. They are the simplest *permanent* Cytods. Their entire body consists of merely soft, structureless Plasson. However thoroughly we examine them with the help of the most delicate chemical reagents and the strongest optical instruments, we yet find *that all the parts are thoroughly homogeneous, each particle in the mass being a mere counterpart of every other particle composing it.* They are, therefore, in the strictest sense, organisms without organs, since they possess no organs, and are not composed of various particles. In a perfectly developed and freely mobile state, *they one and all present us with nothing but a simple lump of an albuminous combination of carbon. The individual genera and species differ only a little in the manner of propagation and development*, and in the way of taking nourishment. They can only be called organisms in so far as they are capable of exercising the phenomenon of organic life, of nutrition, reproduction, secretion, movement. Although in *all real Monera the body consists merely of such a living piece of Plasson, yet amongst the Monera which have been observed in the sea and fresh water, we have been able to distinguish eight different genera and sixteen species, varying in the mode in which their tiny bodies move and reproduce.* The extant Monera afford us organless organisms, such as must have originated by spontaneous generation at the first beginning of life upon the earth. *Even amongst the Monera at present known there is a species which probably even now always comes into existence by spontaneous generation*, namely, the wonderful *Bathybius Hæckelii*. *This has been proved to be* a necessary hypothesis, and is demanded by the requirements of the human understanding for causality. . . . The Monera may be fed with carmine or indigo powder if scattered in the drop of water under the microscope in which they are contained. The grains of colouring matter gradually penetrate the slimy body, and are then driven about in irregular directions. *The smallest separate particles or molecules of the Moneron-*

body, called Plastidules, displace each other, change their relative positions, and thus effect a change in the position of the colouring particles. This *change of position proves positively that a hidden structure does not exist.*' This, Hæckel affirms, '*is a crushing answer* to the assertion that their organization is so minute that in consequence of the inadequate magnifying power of our glasses it is invisible.'*

It will be shown hereafter that the 'crushing answer' referred to by Hæckel is, in reality, no answer at all, inasmuch as a careful comparison of his descriptions and of the evidence furnished by his figures of the most typical Monera, suffice to prove that their body-substance is not homogeneous, even when seen under a very low magnifying power; but, on the contrary, consists of the usual pure basal protoplasm, thickly studded with minute granular particles, which are as integral a part of its composition, as they are of the protoplasm of every true mature Rhizopod without exception. But even were the alleged apparent homogeneousness and total absence of anything approaching to structure or texture in the protoplasm of the Monera a fact, we should be wrong in assuming the non-existence of all organization, inasmuch as it has been proved beyond all question by those indefatigable microscopists, Messrs. Dallinger and Drysdale, that, in a protoplasmic mass given off from the body of certain Monads, in which not a trace of structure or granularity was at first discoverable under the enormous magnifying power of a $\frac{1}{50}$th objective, after the lapse of several hours during which the object had not been lost sight of for a single moment, numberless extremely minute molecules made their appearance, and these proved to be fertilized germs, inasmuch as they continued to increase in size, and after a further lapse of time became developed into the mature parent form from which they had originated; the life-cycle of the species having been once more reproduced from the so-obtained' mature forms. Here, then, we have incontestable evidence that germinal particles—*i.e.*, particles of matter—quite invisible in the earliest stage of existence, *must*, nevertheless, have been present in the protoplasm; and consequently that they must further be regarded as integral and essential factors in the life-cycle of the organisms which gave them birth.

Now, any person examining the figures of *Protamœba* in the accompanying plate (Pl. iv. figs. 1, 2, and 3) will at a glance perceive that they do not convey the impression of a perfectly

* For the full text of the above epitome I must refer the reader to the three latest works in which the questions now referred to are discussed, viz. Hæckel's *History of Creation*, published in 1876, *Evolution of Man*, 1879, and *Freedom of Science and Teaching*, also in the latter year.

homogeneous and structureless substance—such a substance, in short, as the pure albumen of an egg. On the contrary, the figures furnish the most striking evidence that the body substance contains distributed within it *except at the central part*, a multitude of minute but well-defined granular particles, of so nearly uniform size as to warrant the inference that they could not have been introduced accidentally into the bodies of the Monera, but form component portions of their structure. Concerning these particles I shall have something more to say presently. But meanwhile I must invite attention to another equally important proof of organization furnished by these figures.

At the central portion of Fig. 1, Plate iv., a circular space may be noticed, presenting fewer granular particles than the surrounding substance. The boundary of this space, though faint, suffices to prove that, like the granular particles, it is not an accidental portion of the structure. This alone is significant enough, but not so significant as the fact observable in Figs. 2 and 3. Fig. 2, as stated in the description, represents a *Protamœba* 'beginning to divide into two halves.' (It will have been noticed at p. 146, *ante*, that Hæckel speaks of this division as a process of reproduction). The central, comparatively clear space, shown in Fig. 1, has here already divided, one half being retained by and constituting the central space of the upper dividing half of the organism as shown at *a*; the other being retained by and constituting the central space of the lower dividing half as shown at *b*; the central spaces remaining clearly visible in Figs. 3, *c* and *d*, in which division is represented as having been completed.

But, according to Hæckel, 'originally, every organic cell is only a simple globule of mucus, *like a Moneron*, but differing from it in the fact that the homogeneous albuminous substance has separated itself *into two different parts*, a firmer albuminous body, the cell-kernel or nucleus; and an external softer albuminous body, the cell-substance or protoplasm. Besides this, many cells, later on, form a third, frequently absent, distinct part, inasmuch as they cover themselves with a capsule by exuding an outer pellicle, or cell-membrane.' (*History of Creation*, Vol. i. pp. 177–8). Here, then, we have Hæckel himself supplying the requisite data for a correct interpretation of the appearances presented by his *Protamœba*, and, as will presently be seen, his *Protomyxa* and *Myxastrum* likewise. The granules and the central clear space unmistakably indicate an already effected separation of the constituents of the protoplasm into two, if not three, parts, to what degree differing from each other, we have at present no means of accurately ascertaining, except from their external characters. But for our immediate purpose, it is quite sufficient to know that, since

a separation of the constituent atoms of any perfectly homogeneous substance into two or more parts, cannot possibly take place otherwise than through some molecular or chemical change in their original constitution, such substance can no longer be said to be so homogeneous that every individual particle is an exact counterpart of every other particle of the mass. The entire definition and character based upon it is therefore demolished, and with it the foundations upon which the Hæckelian hypothesis of the Monera has been made to rest.

Now in the early stage of every true *Amœba* without exception, granular particles, which undoubtedly constitute an integral portion of the organism even at this period, may be discovered scattered through its substance and taking part in the movements of the protoplasm, arising not from any circulatory faculty resident within it, but from the changes of form undergone by the animal in throwing out pseudopodia for the purpose of creeping or rolling itself along the surface upon which it happens, for the time being, to be moving. This is rendered perfectly certain by the circumstance that when the body ceases to move, the *quasi*-circulation ceases also. In the free-floating Rhizopods, as, for example, the *Foraminifera*, *Polycystina*, *Acanthometræ*, and *Dictyochidæ*, the same thing may be observed, though much less frequently, because in order to bring every portion of the body-substance in turn under the influence of the medium in which the organism lives, a very minute portion being exposed at a time beyond the shell or membranous covering, the vital contractility of the protoplasm is almost continually forcing some of the body-substance out of the foramina, and retracting within the shell a corresponding quantity. But where a separation of the protoplasm has taken place into a clear portion nearly devoid of granules, or, as sometimes is the case, more highly charged with them, it is always possible by dint of a little trouble to perceive that the central mass retains its character unchanged, there being, apparently, no longer a continuous interchange of protoplasmic substance between the central or nuclear portion and that which surrounds it. In the whole of the Rhizopoda the minute granules undoubtedly go to form the true nucleus when this organ is fully developed, and in the lowest order, in which no definite *single* nucleus is present, to form the smaller reproductive organs, to which I gave the name of Sarcoblasts, as being the bodies from which the young of the species are developed, both Nuclei and Sarcoblasts are almost entirely made up of the granules, which are, however, too minute to admit of their structure being resolved. But if the still more minute granules seen by Messrs. Dallinger and Drysdale proved to be the germs

of the parent Monad into which they eventually developed, it is perfectly legitimate to assume, as I did in the case of the Amœban Rhizopods, that the granules are the true reproductive elements of these organisms.

As regards the central clear space in Hæckel's figures of *Protamœba*, it is not requisite to offer many further observations. It is a well-known fact, and one repeatedly alluded to by Hæckel, that in all the more highly organized Protozoa the nuclear body is the first portion of the structure to undergo subdivision. But even allowing for argument's sake that the central space indicated in each figure of *Protamœba* was not intended to represent a nucleus, but some accidental displacement, caused by pressure or otherwise, of the granules pervading the substance of the body, the chances are enormously against such a displacement having taken place in each of the specimens figured. And it is equally improbable that in the representation of the *Protamœba* 'beginning to divide' into two halves, and of the two halves in which division had been completed, any such accidental cause should have led to a central clear space being present in *both* the *new* individuals, precisely as we see in the case of a true nucleus. Such an explanation is, therefore, inadmissible. Hence, as in the case of the manifestly 'differentiated' granular particles, we must regard the appearances as putting beyond all reasonable doubt the fact that the protoplasm of *Protamœba* is not what Hæckel assumes it to be, namely, a substance so homogeneous and structureless that every individual particle of it is the exact counterpart of every other particle composing it, there being no two different portions in the organism. Now the differentiation of living body-substance into two portions is a character on which Hæckel himself very justly lays the greatest emphasis, since it undoubtedly furnishes the most important proof available of advance from a lower to a higher degree of organization. Referring to this, he says:—'We must assume *two very different stages of Evolution in those elementary organisms which, as formative particles or plastids, represent organic individuality of the highest order.* The older and lower stage being that of Cytods, *in which the whole body consists of but one kind of albuminous substance of the simplest plasson or formative material;* the more recent or higher stage being that of cells in which a separation or differentiation of the original *plasma* into two different kinds of albuminous substances, into the inner cell-kernel or *nucleus*, and outer cell-substance or *protoplasm*, has already taken place.'—*Evolution of Man*, vol. ii. p. 45.

But the complicated web of contradictory statements respecting the Monera is not yet exhausted. In his *Evolution of Man*, Hæckel makes the definite assertion that 'the *Monera*, in

their HIGHEST stage of development, consist merely of small pieces of structureless *plasson* or slime;' and that 'the whole body consists merely of *plasson*.' (*Op. cit.* vol. ii. p. 43.)

Let us see how these remarkable statements tally with the history so circumstantially recorded by Hæckel of two of his typical Monera, *Protomyxa aurantiaca* and *Myxastrum radiale*.* It is almost superfluous for me to mention that in the earliest period of independent existence of nearly all the Protozoa holding higher rank than the Rhizopoda, there occurs an Amœboid stage, which is transitory and followed by the other developmental phases constituting the life-cycle of these organisms. Hæckel, on the other hand, draws the primary characters of his Monera—those characters upon which he bases the assertion that they are the simplest conceivable organisms—on the appearances presented by only one out of several phases through which they pass.

After premising that he is about to restrict himself entirely to 'true Monera, *i.e.* naked protoplasmic bodies without nuclei,' and means to pay no attention to the *Protoplasta* distinguished by the possession of one or more nuclei (as *Amœba* and *Arcella*), '*or* to the *Rhizopoda, distinguished by the possession of a distinct shell or membrane*,' we have presented to us a figure of the *Amœboid* '*full-grown fasting*' stage of *Protomyxa aurantiaca* (See Plate iv. fig. 4), an organism detected by Hæckel upon a *Spirula* shell in 1866 at Lanzarote, one of the Canary Islands. Its red colour he regards as distinguishing it from other Monera. He traced it through the following distinct successive stages. The first, that of 'a minute tolerably opaque red ball, covered with *a thick, structureless membrane*,' nevertheless, subsequently found to consist of 'several concentric layers.' The orange-red 'contents of the balls appeared *as a thoroughly* HOMOGENEOUS *obscurely granulated mass, in which might be observed very numerous exceedingly fine particles and a small quantity of strongly-refracting red grains*.' (Plate iv. fig. 5.) Certain indentations on the surface of some of the balls proved to be the external sign of the breaking up of the whole cell-contents into a great number of smaller balls. In some of the balls which were kept under observations in watch-glasses filled with sea-water, the orange-red plasma divided as before into a number of smaller red balls. (Figs. 6 and 7.) But this division did not stop with a repeated bipartite division within the larger cell

* I purposely leave out of consideration Hæckel's Moneron *Protogenes primordialis*, as he admits it to be very closely related to *Amœba porrecta* of Schultze; adding that as the history of its mode of development and reproduction *was unknown to him*, he could not describe its affinities. There can hardly be any doubt as to its being Schultze's *A. porrecta.* Of *Bathybius* as a Moneron nothing need be said.

membrane, but went on, and this, Hæckel observes, '*would be better conceived as a* GERM-*formation than as a process of division or gemmation.*' Still later, some of the smaller or contained balls became pear-shaped and moved actively about when the cell-membrane burst, their movements being accelerated as soon as they were free. (Fig. 8, *e*.) Ultimately, 'the pear-shaped *tail-bearing* GERMS, *or rather germ cytods*' (whatever that may mean), assumed an *Amœboid* condition (Fig. 8, *f*), which Hæckel again declares was structureless throughout, and '*thereby the morphological status of* THE SIMPLEST CONCEIVABLE organic individual was that of a naked cytode or gymnocytode.'

'*Myxastrum radians*' (Plate iv. fig. 9), at first reminded Hæckel of '*Actinosphærium Eichhornii*,' inasmuch as it consisted of a small globular mass of *structureless, homogeneous* jelly, covered with the radiating pseudopodia of *Actinophrys*. The central or inner mass consisted of a sarcode body which contained very numerous interspersed, bright, shining particles, and a small number of larger strongly-refracting granules. *Myxastrum* is distinguished from *Actinosphærium* by the absence of vacuoles, nucleus-holding cells, and any difference between its outer and inner portions, the whole mass being homogeneous. On this ground it might rather be associated with *Actinophrys Sol*. But, again, it does not possess the contractile vesicle of the latter, '*and is especially distinguished by its peculiar reproduction*.' Hæckel attempts to account for the granules by regarding them as '*the products of a change of substance*, it being *extremely probable* that they are assimilated substances produced by the chemical action of the digestive sarcode upon the food taken, and are afterwards changed again into sarcode.' This is supposed to be proved by the increase or diminution in the number of the granules which were observable, as the creatures, whilst under observation, were either fed upon generous diet, or starved. After a time the small globular mass of jelly constituting the body of *Myxastrum* became encysted like *Protomyxa* (Fig. 10), the cyst-membrane becoming thicker and thicker by the addition of fresh concentric layers, and finally reached a thickness amounting to an eighth of the diameter of the entire mass. After the lapse of a fortnight the contents were developed into a number of spindle-shaped bodies (Figs. 11 and 12), which subsequently became oblong and covered by a thin wall having a well-defined double outline, which was *proved to consist of silex* (Fig. 13). Ultimately, the siliceous wall of each of these bodies burst at one extremity, and gave egress to minute spherules—'sporangia' (Fig. 14)—which finally took the parent form of *Myxastrum*.

Here, then, we have presented to us the clearest proof that

typical *Monera,* instead of being 'through life' nothing more than little lumps of the structureless, &c., protoplasm, and exhibiting 'no higher degree of development than that observable in pure protoplasm, undoubtedly pass through several stages in their life-cycle, and that in two at least of these stages, their organization is obviously of a complex kind, even when scrutinized in the most cursory manner. Thus we find they pass through an *Amœboid,* or *Actinophryan*-like stage, a stage of encystment, in which the contents of the cyst—itself a highly differentiated portion of the structure—undergo a process akin to segmentation; each segment, in the case of Protomyxa, emerging from the burst cyst in the shape of a pyriform, caudate, zoospore, which moves about actively, but ultimately takes on the Amœboid form; and in the case of Myxastrum, emerging in the shape of oblong *siliceous* cysts, which on bursting give egress to a little mass of protoplasm, which, as in *Protomyxa,* takes on the Amœboid (Actinophryan) character—a series of facts so significant, that I venture to say they prove incontestably that whatever may be the precise position in nature of the organisms described, they are not inferior in complexity of structure to the Rhizopods.

In short, the error committed throughout Hæckel's observations consists in the determination to regard the protoplasm of the organisms in question as perfectly homogeneous, because he considers this character essential to his hypothesis of their having been formed by spontaneous generation. The researches of Messrs. Dallinger and Drysdale, already referred to, clearly demonstrate this error. In the case of the Monera, which are giants in comparison with Messrs. Dallinger and Drysdale's Monads, the presence of granular particles suffices to upset the hypothesis based upon the absolute homogeneity of their protoplasm, in spite of Hæckel's effort to explain away their undeniable presence on the ground that they are 'the products of a change of substance—assimilated matter, to be eventually changed into sarcode.' For, what are blood-corpuscules, spermatozoa, and every tissue in an animal's body, but products of a change of substance?

Did space permit, it would be easy to show that in the *Amœba,* when immature, the nuclear body not unfrequently escapes observation, owing either to its transparency or to the presence of the granular particles, as well as food-particles, which make their appearance in the sarcode at a very early stage in the life-history of these organisms. The nucleus is, however, always present when *reproductive division* (a term I now use merely to distinguish it from simple repetitive division) has taken place; inasmuch as the former process cannot occur unless a nucleus is present; whereas simple division into

two halves may be repeated almost indefinitely in the absence of a nucleus.

Were proof needed of the difficulty sometimes experienced in detecting the nuclear body, it is to be found in the fact that *Gromia*, one of the most highly developed Rhizopoda, was classed not only *with* the Foraminifera in the lowest order of these organisms until the year 1864, when they were shown by me to be furnished with both a definite, encapsuled nucleus, and a contractile vesicle, but was retained as the *type* of Foraminiferal structure in almost every text-book on the Protozoa.

I may be allowed to say, in conclusion, that I do not pretend to be able to determine the exact position and relations of the structures described as Monera. In order to do this it would be requisite to examine living and authenticated specimens. But I am much mistaken if any such determination is needed to demonstrate that the Monera do not exist in nature as a group of organisms holding an intermediate position between true animals and true plants, as laid down by their describer. They must, accordingly, be henceforth relegated to that limbo of departed spirits to which their illustrious congener, *Bathybius*, has already preceded them—unless, indeed, we are to accept another of the extraordinary conclusions arrived at by Professor Hæckel in relation to the Monera, viz., that '*the conception of an independent soul-life to every individual organic cell is a validly proved conception, by the study of Infusoria, Amœbæ, and other one-celled organisms; for in these individual isolated living cells, we find the same manifestations of soul, life, feelings, and ideas (mental images), will, and motion, as in the higher animals compounded of many cells.*' (*Freedom of Science and Teaching*, p. 47.)

EXPLANATION OF PLATE IV.

The whole of these figures are, as nearly as possible, facsimiles of Hæckel's originals.

FIG. 1. *Protamœba primitiva*, a typical freshwater Moneron, in the free Amœboid condition.

FIG. 2. The same beginning to divide. It will be seen that the central clear space visible in fig. 1, has already divided, and is visible both at *a* and *b*.

FIG. 3 The same, division being complete; and the two separate individuals each presenting a central clear space.

FIG. 4. *Protomyxa aurantiaca*, another typical Moneron. 'A full-grown specimen' said to be 'fasting.'

FIG. 5. The same 'encysted.'

FIG. 6. The same, more developed. 'The homogeneous protoplasm has

retracted itself from the inside of the wall of the cyst, which has become thickened, and has begun to divide into numerous small globular bodies. Between the plasma-ball and the outer covering a little clear fluid has collected.'

FIG. 7. The same further developed.

FIG. 8. 'The small globular bodies have now drawn themselves out into a long tail, and issue from the cyst as *swarm-spores*, with a lively motion.' At *e*, 'the pear-shaped spores, having ruptured the cyst, move about freely with the aid of their tails.' At *f*, some 'spores becoming stationary retract their tails and protrude pseudopodia,' by means of which they creep about after the manner of *Amœbæ*. Fig. 8, *g*, shows 'three of these Amœboid germs united into a single plasmodium;' vacuoli being now for the first time observable.

FIG. 9. *Myxastrum radians*, another typical Moneron in the Amœboid or Actinophryan stage: not quite full-grown.

FIG. 10. The same after arriving at full growth, encysted, 'showing the homogeneous colourless ball of protoplasm, surrounded by a *tough, structureless, gelatinous*, covering.'

FIG. 11. The same, 'at commencement of development, dividing by *radial cleavage* into numerous conical portions.'

FIG. 12. The same, still further developed. 'The cone-shaped masses have now assumed a spindle-shape, and each separate one has developed a *siliceous* covering.'

FIG. 13. The empty siliceous shell of one of these '*spores*.'

FIG. 14. 'A spore, showing the protoplasmic contents escaping from the siliceous shell.'

REVIEWS.

BRITISH MARINE POLYZOA.*

EVERY naturalist knows the magnificent series of works on British Zoology for the publication of which we are indebted to the liberality and enterprise of Mr. Van Voorst. The Quadrupeds, Reptiles, and Stalk-eyed Crustacea of the late Professor Bell, whose recent death at a ripe old age is deplored by all who knew him; Yarrell's Birds and Fishes; the History of British Echinodermata, by Professor Edward Forbes, and the splendid work on the Mollusca, by the last-named writer and Mr. Sylvanus Hanley; and MM. Spence Bate and Westwood's history of the Sessile-eyed Crustacea, are all standard works, covering a great part of the domain of British Zoology, and reflecting the highest credit both upon their authors and upon the publisher who has brought them out so handsomely printed and so admirably and lavishly illustrated. Besides the above-mentioned works, Dr. George Johnston, of Berwick, one of the best naturalists of his day, contributed to this series in 1849 a second edition of his History of British Zoophytes (originally published in Edinburgh in 1838), in which he included the whole of the animals to which the term Zoophyte was then commonly applied, namely, the Sea-anemones and Corals, and the Alcyonarian and Hydroid Polypes, belonging to the division Cœlenterata of more recent zoologists, and the Polyzoa, or so-called Ascidian polypes of some authors of that day, which had been regarded as for the most part allied to the hydroid Sertularians by the older zoologists, until the distinction between the two forms was pointed out by Grant and Milne-Edwards, followed about 1830 by Vaughan Thompson and Ehrenberg, and a little later by Dr. Arthur Farre.

As a matter of course, in the thirty years which have elapsed since the publication of the last edition of Dr. Johnston's work, much has been done in the study of a group of organisms so interesting to zoologists as these lowly creatures. Many new forms have been discovered, many points in their history have been cleared up, and new, unexpected, and most remarkable phenomena have been observed by the unwearied investigators of the last quarter of a century. Mr. Gosse's useful and beautifully-illustrated History of British Sea-anemones and Corals was the outcome of the general attention paid to these flowers of the sea in consequence of the setting in of the fashion

* *A History of the British Marine Polyzoa.* By Thomas Hincks, B.A., F.R.S. 2 vols. 8vo. London, Van Voorst, 1880.

of keeping aquaria; the Alcyonarian zoophytes are still wai
historian, but the remainder of the forms properly included
Zoophytes by Johnston and the older naturalists, have now n
treatment at the hands of the Rev. Thomas Hincks, whose v
on the British Hydroid Zoophytes appeared in 1868, and is n
a splendid work on the Marine Polyzoa. The few fresh-wa
omitted, as having been made the subject of a special treatise
published by the Ray Society.

The Polyzoa, which include among others the well-know
mats, and many of the slender, branching, horny zoophytes
the composition of the so-called sea-weed baskets and pi
visitors to our watering-places, were regarded by Linnæus
down to the time of Lamarck and Cuvier, as very near
Sertulariæ and other Hydroid polypes, the last-named aut
edition of his *Règne Animal* (1829), only separating the
(his *Polypes vaginiformes*) as a distinct family under the
à cellules,' the distinction being founded on differences of
the animals in both cases said to resemble *Hydræ*, a
simplicity of the Sertularian polype seems already to
But at an earlier period, it had been noticed that th
the animals of different species of so-called *Sertular*
able. Thompson's observations were not published un
that they were made soon after 1820. He called
form of polype a 'Polyzoa,' in opposition to the te
the individual zooids of the true Sertularians and
meantime, however, the distinction had been noticed
Edwards; the fact that the new type of structure co
that of the Ascidians was recognized, and shortly aft
removed from the division of Zoophytes to that
placed in the immediate vicinity of the Ascidians.
been considerable discussion as to the propriety
assigned to the Polyzoa, several eminent authorities be
them to the Vermes, under which rather miscellaneous
to find their allies; whilst others still maintain their n
the latter opinion Mr. Hincks adheres, and he strengt
adducing what appears to be a strict homology betw
the embryo of the bivalve Mollusca.

Another matter about which there has been a
dispute is the name which ought to be given to
Thompson's memoir was published in December, 18
appeared the first part of Ehrenberg's *Symbolæ*
Prussian naturalist established a class, 'Bryozoa,' f
Polyzoa and of some other organisms, which he ass
a mistaken notion of their affinities. The question
which of these names should be adopted. On the m
there need be no doubt, always assuming that Thom
tion of a new group of zoophytes under the nam
'Bryozoa,' if admissible on other grounds, wi
applied to the class. The objection raised to 'Po

being
; there is no
themselves seem
by degrees to the
earth on which we live
by the energy of the
ocean. The 'Urzelle'
basalts and lavas of

finding similar manifestations of his 'Urzelle' in all sorts of rocks, belonging to all sorts of series, in granite, in Carrara marble, in various crystalline rocks, in basalt, and finally in meteoric stones and meteoric iron. Not unnaturally the forms of the plants are numerous and diversified, and the author distinguishes a host of genera and species, in the names of most of which we find traces of the same carelessness as in the case of *Eophyllum*. These simple cellular Algæ are described as presenting points of structure which belong to the most dissimilar groups of plants; they have stems, buds, and prothallia, the stems and cells are seen in conjugation, &c.

We may cite a few of the names conferred upon these wonderful organisms. One is named *Ophthalmia Hochstetteri*, another *Dufferinia*, in honour of Lord Dufferin, and another *Victoria*, in honour (?) of the Queen of England; Mr. Darwin is complimented by being made to stand sponsor to a *Marmora Darwini*, the Crown Prince of Austria has a *Stygia* named after him; while the German Emperor is honoured by having his name attached to a species coming from above instead of below, namely, *Urania Guilielmi*, discovered in the meteoric storm of Knyahinya.* Two people upon whom this questionable compliment is conferred come off rather worse than the rest, and one of these is no other than Prince Bismarck, 'unser Reichskanzler.' 'The first animal' is named *Titanus Bismarki*, in honour of this distinguished individual; it is said to be a serpuliform worm, and its resemblance to the illustrious chancellor of the German Empire would appear to consist in its wearing a cuirass (*Kieselpanzer*) and being of a rather paunchy figure. It is to be hoped that Prince Bismarck can enjoy a joke even at his own expense, otherwise Dr. Hahn may find cause to repent him of this one. The other name is worse, and while its presence here seems to be conclusive as to the ironical character of this book, its use is, we think, most strongly to be reprobated. Dr. Hahn names one of his species, obtained from flint-nodules which he erroneously supposed to be from the Silurian of Canada, *Photophoba Dawsoni*, 'Dawson's Light-dreader,' we presume, or it might be 'Dawson's dread of the light:' and as if this was not a sufficient affront, he expresses a hope that over this delectable piece of false wit he and Dr. Dawson may shake hands in token of reconciliation!

But we have not yet quite done with Dr. Hahn. The essence of an ironical work, such as we believe this to be, is to start with absurd propositions and work them out as logically as possible to their extreme consequences; and thus, out of the discovery of *Eophyllum canadense*, there springs under the author's pen a new theory of rocks, and indeed of the formation of the earth, and all that it contains. These cellular plants that we have been discussing are by no means to be regarded as fossils in the ordinary sense of the word; they are, in most cases, the actual producers of the rocks in which their traces occur; even the crystalline rocks not being exempt. 'Granite,' we are told, 'is nothing but a plant-mass: there is no rock-mass with it; all is plant!' The crystals of minerals themselves seem to represent the cells of the plant! Hence we are led by degrees to the recognition of the startling fact that the crust of the earth on which we live is a sort of Sargasso bed, built up in the course of ages by the energy of the 'Urzelle,' and floating upon some kind of primordial ocean. The 'Urzelle' itself is proved to be still at work by its occurrence in basalts and lavas of

recent geological date; hence its growth does not require the presence of light and air, but only of moisture and heat, and we have here a remarkable coincidence with the eruptive phenomena which have brought the later manifestations of these marvellous organisms to the surface, and with the known distribution of volcanoes at no great distance from the sea!

We have thought it worth while to give a sketch of the general contents of this book, both because it has been regarded as written with a serious intention by critics who have blamed the author for burthening science with such a mass of absurdly useless names, and because it is in itself a curious and noteworthy phenomenon. Its style is compared by one of its German critics to that of the Book of Revelations, and it must be confessed that there is some similarity. Had the author written one-third of the quantity, and illustrated his pamphlet with only two or three plates, it would have told much more powerfully against his opponents. Dr. Hahn is not quite a Swift, and even the great Dean's irony was not appreciated by everybody.

We understand that Professor King has a work in preparation which will bear upon this subject; it will be entitled 'An Old Chapter of the Geological Record with a new Interpretation: or Rock-metamorphism, especially Methylosis, and its resultant imitations of Organisms. With an Introduction giving a history of the controversy on the so-called *Eozoon Canadense*.'

Archæopteryx macrura.—At the meeting of the Swiss Society of Natural Sciences, held at St. Gall in August, 1879, Professor Carl Vogt read a most interesting communication upon the second specimen of this remarkable fossil organism, which was found in the lithographic slate of Pappenheim some years since, but of which no naturalist had hitherto been able to make a careful examination.

The genus *Archæopteryx* was established in 1861 by Hermann von Meyer, upon the evidence of a feather found fossil in the lithographic stone of Solenhofen. The species was named *Archæopteryx lithographica*, in allusion to the formation in which it was found. Somewhat later a much more important specimen was found by a M. Haberlein in the same deposits. This consisted of a slab of limestone containing various parts of a feather-bearing creature, including limb-bones, vertebræ, traces of the skull, and especially a long, slender tail, from each vertebra of which sprang a pair of quill-feathers. This slab was subsequently purchased for the British Museum, and in 1863 Prof. Owen described it as a species of Von Meyer's genus under the name of *Archæopteryx macrura*, in allusion to the length of the tail. Notwithstanding this remarkable peculiarity, *Archæopteryx* was referred to the class of Birds, but placed in a special group, to which the name of *Saururæ* was given. The animal has always excited the greatest interest, from its apparent combination of reptilian and avian characters, a circumstance which acquires the more importance from the occurrence of the fossils in beds which are so rich in Pterodactyles. It seemed that we had here in the same deposit the most bird-like of Reptiles and the most reptile-like of Birds.

The second specimen, discovered by M. Häberlein's son in the lithographic stone of Pappenheim, is now, we believe, in the Senckenbergian Museum at Frankfort; but for some years after its discovery no one was allowed to see

it. According to Prof. Vogt's account of it, it is nearly complete, and its wings are unfolded as if in flight. The head is small. Implanted in the upper jaw two small conical teeth may be detected with a lens. Eight cylindrical cervical vertebræ, furnished with very fine ribs directed backwards, were counted. The dorsal vertebræ appear to be ten in number, thick and short, and destitute of spinous processes. Their ribs are slender, curved, and pointed at the end; they show no flattening, nor are there any traces of the uncinate processes which in most birds spring from the posterior margin of each rib and rest against the succeeding one. There are also very fine sternal ribs, which appear to be attached to a linear abdominal sternum. The pelvis is still enclosed in the matrix. The tail is very long, and agrees with that of the first known specimen. The hind limbs are hardly so perfect as in Prof. Owen's example, but they show with certainty that the fibula is completely united to the tibia, and only marked off by a not very strong longitudinal furrow. The anterior limbs, on the contrary, are more perfect than in the original example. M. Vogt thinks that two scapulæ are recognizable, and that there is no bone representing the furculum. The two coracoids seem to be in contact in the median line, and the sternum is reduced to zero. The bones of the arm present no features peculiar to reptiles or to birds; they have been already well described by Prof. Owen. But the bones of the hand in the new specimen show characters not previously known. The carpus shows only one small globular bone. The digits are well preserved in both limbs; and from the information now obtained it appears that the hand of the *Archæopteryx* cannot be compared to that of a bird, or to that of a pterodactyle, but only to that of a tridactyle lizard. There are three long, slender digits, armed with curved and sharp-edged claws. The thumb is the shortest, and is composed of a short metacarpal, a long phalange, and the ungual phalange. The other two digits consist of a metacarpal and of three phalanges. The wing-feathers were attached to the cubital margin of the fore-arm and hand, but no special adaptation of the skeleton to this purpose can be observed The thumb was free, like the other two digits, and did not support a winglet. Thus, if the feathers had not been preserved no one could have suspected from the examination of the skeleton that the animal had been furnished with wings.

Prof. Vogt sums up the information we now possess as to the organization of *Archæopteryx*, with special reference to the question of its systematic position. The head, neck, thorax and ribs, tail, shoulder girdle, and the whole anterior limb, are clearly constructed as in Reptiles: the pelvis has probably more relation to that of Reptiles than to that of Birds: the posterior limb is that of a Bird. The reptilian homologies certainly preponderate in the skeleton. There remain the feathers: these are unmistakably birds' feathers, with a median rhachis and with perfectly formed barbules. The new slab shows all the feathers in their place. The remiges, as already stated, are attached to the cubital margin of the arm and hand; they are covered for almost half their length with a fine filiform down; none of them project beyond the others, and the wing is rounded in outline, like that of a common fowl. There are thought to be indications at the base of the neck of a collar like that of the condor. The tibia was covered throughout with feathers, so that the leg of the *Archæopteryx* must have presented an external

resemblance to that of a falcon, as also, according to Prof. Owen, in its anatomical structure. Each caudal vertebra bore a pair of rectrices, but all the rest of the body was apparently destitute of feathers, otherwise traces of them must have been found upon a slab which has preserved even the smallest details of a fine down.

Prof. Vogt regards *Archæopteryx* as neither a reptile nor a bird. It constitutes according to him an intermediate type of the most strongly marked description, confirming the views of Prof. Huxley, who has united reptiles and birds, under the name of Sauropsida, as forming a single great section of the Vertebrata. It is one of the most important signposts on the road which has been followed by the class of birds in its gradual differentiation from the reptiles from which it originated. A bird by its integument and hinder limbs, it is a reptile in all the rest of its structure; and thus its conformation can only be understood if we accept the evolution of birds by progressive development from certain types of reptiles. The Cretaceous birds described by Prof. Marsh indicate a later step in the same direction.

In discussing the stages by which *Archæopteryx* passed to arrive at the form under which we know it, and the mode in which adaptation for flight has acted upon the different parts of the body, Prof. Vogt shows, in the first place, that this adaptation in Vertebrata is by no means necessarily combined with that of an upright position. This is seen in the Pterosauria and the Bats. The structure of the hind feet that occurs in the Dinosauria, the *Archæopteryx*, and in birds, is therefore, he thinks, independent of the faculty of flight, and only stands in relation to the possibility of sustaining the body upon the hinder feet alone; and hence the resemblance in this respect between Dinosauria and birds by no means indicates real affinity. At the utmost he would regard it as possibly indicating a genetic connexion between the Dinosauria and the Struthious birds, but this would involve a multiple origin of the class of birds.

The search in deposits older than the Upper Jurassic for reptiles which may be related to *Archæopteryx*, and thus indicate earlier stages in the process of evolution, would seem to be vain, since the fossils we possess are destitute of tegumentary parts, and it is very difficult to say *à priori* with what cutaneous structures these creatures were covered. At the same time, as Prof. Vogt points out, there is complete homology between the scales or spines of reptiles and the feathers of birds. The reptilian structures, he remarks, differ in no respect from the wart-like stumps which appear in the embryo bird as the first traces of plumage; the feather of the bird is only a reptile's scale further developed; and the reptile's scale is a feather which has remained in the embryonic condition. The feathers of *Archæopteryx*, which are so perfect, must undoubtedly have been preceded in some pre-existing reptiles by cutaneous structures representing persistently the different stages of the embryonic development of the feathers. We may, therefore, imagine the ancestors of the *Archæopteryx* as lizard-like, terrestrial reptiles, having feet with five, hooked, free digits, showing no modification of the skeleton, but having the skin furnished at different points with elongated warts, downy plumes, and rudimentary feathers, not yet fitted for flight, but susceptible of further development in the course of generations.—(*Bibl. Univ.*, December 15, 1879; *Ann. and Mag. Nat. Hist.*, February, 1880.)

Correlation of the Rocks of the South of Ireland with those of other Districts.—Prof. Hull, Director of the Geological Survey of Ireland, read an interesting paper before the Geological Society on this subject, on March 10th. He referred to a previous paper in which he had discussed the geological age of the Glengariff (or Dingle) beds, and arrived at the conclusion that these are Upper Silurian (Upper Ludlow) deposits, greatly expanded vertically. These rocks were disturbed, elevated, and denuded, and ultimately submerged in such a manner, that the later Old Red Sandstone and Lower Carboniferous strata are found resting transgressively upon them. In comparing these Irish rocks with those of North Devon, Prof. Hull first showed that the true division between the Old Red Sandstone and Carboniferous series must be taken at the top of the 'Kiltorcan Beds' with the freshwater bivalve *Anodonta Jukesii*. He held that the Coomhola grits and Carboniferous slate series of the south of Ireland are represented in Devonshire by the Barnstaple, Pilton, and Marwood beds, thus relegating these divisions of the Devonshire geologists to the Carboniferous. This conclusion is borne out by palæontological evidence.

The Upcot Flags and Pickwell Down Sandstone are to be regarded as the representatives of the Kiltorcan beds and Old Red Sandstone and Conglomerate of the south of Ireland; but here the correlation of the formations of the two districts ends, for the Martinhoe, Ilfracombe, Hangman, and Lynton beds have no representatives in the latter locality. These groups, the Lower and Middle Devonian marine deposits, were deposited during the upheaval of the Irish Upper Silurians.

The Foreland Grits, which lie at the base of the whole Devonian series in Devonshire being identified by him with the uppermost Silurian, or 'passage-beds' of Murchison, the author inferred that the great gap existing in Ireland between the Glengariff beds and the succeeding Old Red and Carboniferous series, was filled up in Devonshire by the beds lying between the Pickwell Down Sandstone and the Foreland Grits.

Accepting Prof. Geikie's suggestion, that the Scotch 'Lower Old Red' is the representative of the Irish Glengariff beds, Prof. Hull concluded that this 'Lower Old Red' is really the lacustrine equivalent in time of the marine uppermost Silurian strata (a view which is supported by fossil evidence); while the 'Upper Old Red' of Scotland is the equivalent of the formation bearing the same name in Ireland. Hence it follows that there is only one formation properly called 'Old Red Sandstone,' and this is the equivalent of the Upper Devonian as restricted to the Pickwell Down Sandstone of Devonshire.

In the district north of the Severn, embracing the region of 'Siluria,' the limestone shale, both by its position and fossils, is shown to be the greatly reduced representative of the Barnstaple, Pilton, and Marwood beds; while the Yellow and Red Sandstone and Conglomerate represent the Pickwell Down Sandstone, or true Old Red Sandstone. Between this and the Upper Ludlow and Passage-beds comes the Cornstone group, to which Mr. Godwin-Austen and Prof. Ramsay have assigned a lacustrine origin. Prof. Hull is of opinion that the thickness of this group has been greatly over-estimated, and regards 5000—6000 feet as the maximum. It was probably deposited in an estuary opening seawards towards the south. He accepted

the conclusion, suggested by M. Dewalque, that the Cornstone group represents the Middle and Lower Devonian groups of Devonshire, lying between the Old Red Conglomerate and the Upper Silurian. The Belgian 'Psammites du Condroz,' a great group of sandstones lying between the Carboniferous Limestone and the 'Calcaire du Frasne,' is to be regarded as the representative of the Pickwell Down Sandstone, and therefore of the true Old Red, notwithstanding the presence in it of marine fossils.

The principal conclusions to be drawn from these arguments are as follows: There is only one Old Red Sandstone properly so called, represented in Devonshire by the Pickwell Down Sandstone, and in Scotland and Ireland by the so-called Upper Old Red, in which case the Old Red Sandstone is not the equivalent of the marine Devonian strata, but surmounts them. Secondly, at the close of the Upper Silurian period, all the western and northern portions of the British Isles were disturbed and elevated into land surfaces, while subsidence and deposition of marine strata were taking place over the south of England and Wales, and adjoining parts of western Europe, the Lower and Middle Devonian groups being then formed. At the commencement of the Upper Devonian stage there was a general subsidence; lakes were formed over the Irish and Scottish areas, on the shelving shores of which the Old Red Conglomerate was accumulated; and at the commencement of the Carboniferous period a further subsidence took place, bringing in the waters of the ocean over all the submerged areas.

MINERALOGY.

Hedyphane containing baryta from Laangban.—Lindström describes (*Jahrbuch für Mineralogie*, 1879, 896) this specimen as white to pale yellow-white, and except as regards its showing the barium reaction and higher specific gravity of 5·82, according in every respect with hedyphane. The analyses given below show, under I., the composition as found, and, under II., the insoluble portion subtracted as well as the lime carbonate and a quantity of oxygen equivalent to the chlorine.

	I.	II.
Arsenic acid	28·18	29·01
Phosphoric acid	0·53	0·55
Carbonic acid	1·07	Lead 9·19
Chlorine	3·05	3·14
Lead oxide	49·44	41·01
Baryta	8·03	8·27
Lime	8·99	7·85
Magnesia	0·24	0·25
Iron oxide	0·08	0·08
Soda	0·15	0·15
Potash	0·09	0·09
Insoluble residue	0·42	—
	100·27	99·57

Excluding the iron oxide, potash and soda we have the general formula,

$$3\ (3\ RO,\ As_2\ O_5) + Pb\ Cl_2$$

Herrengrundite, a new basic Copper sulphate.—This body is described by Brezina as occurring in little plates, 1 to 2·5 mm. in diameter, scarcely 0·2 mm. in thickness, and having apparently the composition indicated by the numbers—

Sulphuric acid	23·04
Copper oxide	57·52
Water	19·44
	100·00

2·05 lime found in the analysis have been subtracted, as well as the requisite amount of sulphuric acid and water to form gypsum with it. The colour of the plate-like crystals is dark emerald-green, the hardness, 2·5: it is almost brittle, the cleavage along, oP (001) complete. The axes are,

$$a : b : c = 1·8161 : 1 : 2·8004$$

Titanomorphite. (*Jahrbuch für Mineralogie*, 1879, 897.)—Von Lasaulx directs attention to the widely-spread and characteristic product of the decomposition of menaccanite, which occurs in rocks, and which he has always held to be a lime titanate analogous to perowskite in point of composition. He has recently met with it in larger quantities. In a slaty amphibole rock from the gneiss of Lampersdorf, near Reichenbach, in Silesia, occur rounded nodules of rutile which are covered with a greenish or yellowish-white zone of a granulated or fibrous product of decomposition. Sufficient of this material was collected for analysis, and found to consist of

Lime	25·27
Titanic acid	74·32
Iron protoxide	Trace
	99·59

These numbers correspond with the formula,—

$$Ca\,O_1\ 2\,Ti\,O_2$$

which is that of the new mineral. (*Jahrb. fur Mineralogie*, 1879, 569.)

A new Barytic Felspar.—Des Cloizeaux found in the collection of the *Muséum d'Histoire Naturelle* a few fragments of a felspar prism, of unknown locality, which are transparent or only translucent, and resemble the albite of St. Vincenz, Styria. The two cleavage directions, oP (001) and ∞ P ∞ (010) form an angle of 86° 37′, and the reentering angle of the base was found to be 173° 14′. These values approach closely those found for labradorite, while the optical characters of the felspar in question more closely agree with those of oligoclase and albite. To decide this question, the mineral was analysed, and the following results arrived at:

Silicic acid	55·10	8
Alumina	23·20	3
Iron peroxide	0·45	
Baryta	7·30	1
Lime	1·83	
Magnesia	0·56	
Soda	7·45	
Potash	0·83	
Loss by heating	3·72	
	100·44	

While the analysis agrees with that of a barytic andesine, the mineral shows by its optical properties and the angle of its cleavage planes, that it is not that; in fact, it agrees with none of the felspars which have yet been examined. (*Jahrbuch für Mineralogie*, 1879, 591.)

PHYSICS.

The Thermo-electric behaviour of Aqueous Solutions with Mercurial Electrodes, has been communicated to the Royal Society by Mr. G. Gore. He used for this purpose an apparatus consisting of two thin glass basins, containing each a layer of mercury about one centim. deep, covered by a layer three centim. deep, of the aqueous solution to be examined. A bent glass tube of the shape of an inverted U with a vertical canal at the highest point was filled with the liquid, and inverted over the mercury; two stout glass tubes with a platinum wire sealed into their lower ends, one filled with mercury, were placed in the mercury of the basins before adding the liquid. Terminals of platinum touched the mercury, but not the liquid. Thermometers were immersed in either pool of mercury. One basin was supported on wire gauze with a Bunsen's burner beneath it, the other on a shelf at the ordinary temperature. The mercury was carefully purified. A galvanometer was used to test the completeness of the circuit. One basin was then heated, and the deflection of the galvanometer noted at various temperatures. Solutions were chosen which had little chemical action on mercury, and which were free from any visible film or dulness over the heated metal. The galvanometer was astatic with two coils, each of 50 ohms resistance, connected together. A table was obtained with the solution at the top, in which the hot mercury was most positive at 180° Fahr., and that at the bottom in which it was most negative. Far at the head stands phosphate of ammonium, with a deflection of + 28° followed by carbonate of sodium with + 19·0. At the other extremity of the scale stands cyanide of potassium, which sinks from − 14°·0 in a solution of 5 grs. to − 50°·0 in one of 100 grs. In order to ascertain whether this order agreed with that of a series arranged according to the different degrees of electromotive force of the various couples, two similar basins were charged with the two solutions and connected with a differential galvanometer with currents in opposite directions, to ascertain which gave the strongest currents.

On examining the results it is observed that the effects are not manifestly related to the chemical nature of the solutions. It is difficult to prove how far chemical action occurred as a cause of currents, though it cannot have been great. The strength of the solution clearly affected the quantity, and in some instances the direction of the current. Stirring, and previous heating of the solution, also influenced the deflection. It appeared that the currents were due to heat acting on and altering particular molecular structures, and that the difference of electromotive power was not due to chemical variety, but to differences of molecular arrangements.

Measurements by means of Thomson's Galvanometers are not, according to M. Gaiffe, as reported in the *Comptes Rendus* for January, found to be proportional to the value of the currents measured, but to be exaggerated as those values increase. This error arises from the fact that the angles of

deflection of the magnet are doubled by the reflection of the mirror, and that consequently it is not the tangents of the real angles of deflection that one reads on the scale, but the tangents of the doubled angles. This assumption may indeed be made with very small angles, but even with a deflection of 8° the indications are already sensibly exaggerated at the ends of the scale. A bifilar suspension with two silk threads very close together reduced the erro s to less than one hundredth of the value measured.

Distribution of power by Electricity has formed the subject of experiments by M. Tresca, using a Gramme machine making 1200 revolutions per minute. The current was transmitted by copper-wires to carts at different distances, two Gramme machines being placed on the carts, and acting on a windlass which drove a double Brabant plough so as to make a furrow 220 metres long. The velocity of the plough when the circuit was 800 metres, was 88 centimetres per second, the shaft making 1123 turns per minute; when the circuit was increased to 1300 metres the velocity was 70 centimetres, and the revolutions of the shaft 890. The effective work was estimated at three-horse power.

Electrical Storage has been described by Professors Houston and Elihu Thomson in the *Journal of the Franklin Institute*. Such a process has long been suggested, but has failed to become of practical use, (1) from cumbrousness, owing to the large conducting surface required; (2) from loss of energy due to evolution of gas during charging; (3) lack of constancy; (4) limited capacity. These defects apply to Planté's secondary battery; the system they suggest consists of a saturated solution of zinc sulphate, at the bottom of which is placed a plate of copper connected to an insulated wire. At the top is placed a second copper plate, one of hard carbon, or of some metal unchanged by contact with zinc sulphate solution, and less positive than metallic zinc. Charging of the cell so constructed is effected by passing a current through it from the lower to the upper plate. The current employed is that from a dynamo-electric machine, and the result is the deposition of metallic zinc on the upper plate, and the formation of a dense solution of copper sulphate overlying the lower. The cell, in fact, becomes a gravity battery, acting as such, till all the deposit is redissolved.

A somewhat similar plan has been employed by Mr. C. F. Varley to distribute time-signals, but has been superseded by Leclanché cells of large dimensions. It consisted of gutta-percha chambers 4½ in. high, each with a division of 1¼ in. deep in the centre; the shallow cells thus formed being half filled with mercury, and connected with the mercury in the corresponding cell of the neighbouring chamber. The last carbons were connected to terminals and to the discharging wires; the cells were filled with zinc sulphate and sulphuric acid. On passing a current through sixty such cells from 150 small Daniels, amalgamated zinc was formed on one side and bisulphate of mercury on the other. The arrangement had a very low resistance.

Cast-Iron Magnets are being made by M. Carré by running a soft and slightly carburetted metal in earthen crucibles. Just before pouring, 10 to 15 per cent of steel-filings and about 1 or 1·5 of nickel with ·25 per cent of copper; or 2 per cent of tin and 0·5 per cent of copper, are added. It can then stand tempering at a cherry-red heat.

Nature extracts from the *New York Nation* a marvellous account of

an instrument which its inventor, Prof. Alfred W. Mayer, terms the 'Topophone,' or Sound-place. 'It consists of a vertical rod passing through the roof of the deck cabin, bearing on its upper end a horizontal bar, carrying two adjustable resonators, below which a pointer is set at right angles with the bar. Rubber tubes from the resonators pass through the roof of the cabin, and unite in a single pipe connected with a pair of ear-tubes. The vertical rod is turned, by means of a handle, in any direction. The first step is to tune the resonators accurately to the pitch of the sound under observation, and the second to fix them 'at a distance from each other somewhat less than the length of wave of that sound; next, by turning the handle to bring them simultaneously on the wave-surface, when, as they both receive at the same instant the same phase of vibration on the planes of their mouths, it will result that, if the connecting tubes be of the same length, the sound-pulses acting together will be reinforced to the ear; but if the tubes differ in length by one half the wave-length of the sound, the pulses will oppose and neutralize each other. At this moment the horizontal bar is a chord in the spherical wave-surface, of which the distant fog-horn is the centre, and the pointer represents a radius directed to the place from which the sound emanates.' This seems a pretty and useful little problem to set to a skipper in a gale and a fog, with an unknown danger-signal hardly audible in the offing. It is just possible that he might be better employed on deck.

Dynamo-electric Machines for Telegraphic Purposes, are described in a late number of the *Scientific American*. They are intended to replace 14,300 gravity battery elements, and 4600 bichromate of potash cells. The machines are on Siemens's system, connected in series with their field-magnets excited by a current from a single Siemens machine. One commutator-brush of a machine is connected with the brush of opposite polarity in the next, and so on, so that a current of any desired potential may be had from the different machines in the series. The E.M.F. in the first being 50 volts, the second will be 100, and the third 150. A patent to this effect was taken out by Mr. H. Wylde in 1878 in this country. Dr. Schwendler, Electrician to the Indian Government, finds the dynamo-electric current better for telegraphic purposes than those from batteries. A signal current can be obtained from that maintaining a powerful light by derivation without perceptibly diminishing the lighting power.

Atmospheric Polarization and the influence of Terrestrial Magnetism on the Atmosphere form the subject of a memoir in the *Annales de Chimie et de Physique*, by Henri Becquerel. First observed by Arago in 1809, it was further worked out by Babinet, Brewster, Bernard, Liais and Rubenson. If the light emitted by the atmosphere in the sun's vertical plane be analysed at an hour when the luminary is at a small elevation above the horizon, it is found to be faintly polarized in his neighbourhood, the polarization increasing towards the zenith. The maximum occurs at an angle of 90° from the sun, falling again to zero at a point named Arago's neutral point. Up to this the plane of polarization is vertical, below it it is horizontal. At sunrise and sunset, the neutral point is from 20° to 30° above the horizon. Babinet found a second above the sun, and Brewster a third at about the same distance below. There seem to be other secondary neutral points related to those above named, and discoverable in certain states of the atmosphere.

The explanation has generally been referred to reflection of the solar rays for the vertical, and to refraction joined to secondary reflections, for the horizontal polarization. The neutral points thus become spots where the vertical and horizontal actions have equal and opposite intensity. Hypotheses regarding the method of reflection have been founded on the partial opacity of the air itself, on that of suspended solid particles, and on small vesicles of water. Hagenbach referred it to layers of air of unequal density. Tyndall considers it not due so much to simple reflection as to a peculiar phenomenon which attends on luminous gas.

Wheatstone, by means of an apparatus which he termed a Polar Clock, showed that by pointing this to the pole the position of the plane of polarization would approximately give solar time. M. Becquerel set himself to determine with precision the relative positions of the sun and of the plane of polarization.

The apparatus he devised for these researches consists of a Savart polariscope mounted in a divided circle, and observed by means of a total reflection prism. It can be turned in any direction, azimuth and altitude circles giving independently the co-ordinates of the point observed with reference to the magnetic north.

The observations consisted in determining on the same divided circle the position of the plane of polarization, and the shadow of the optical axis of the apparatus thrown by the sun itself. Cross wires were fixed to the end of the movable tube which threw an image on a plane surface attached to it. These were afterwards replaced by two needle-points which threw their shadow on a small screen regulated to move in the sun's plane. The Savart polariscope was found more delicate than others depending on equality of tint in the presence of much diffused non-polarized light; but instead of observing the greatest intensity of the fringes, their disappearance at an angle of 45° was noted, which proved more susceptible of accurate determination.

The process of observing consisted in taking three double measurements of the solar plane, with the time. Then a series of observations of the plane of atmospheric polarization followed, and then again a repetition of the first measurements. These were plotted out with lines as Abscissæ and the numbers found as Ordinates. The former proved the more accurate. Several of them are reproduced in the memoir.

The results, stated briefly, are as follows: 1. The plane of polarization from any given point in the sky does not generally pass through the sun, but usually below it. At the zenith, the planes of the sun and of polarization coincide. At the pole their angle is small, and was hence unnoticed by Wheatstone. They increase towards the horizon. As a rule the polarized light coming from the sky behaves like a luminous ray starting from the neighbourhood of the sun and reflected towards the observer; but it contains rays polarized by refraction also, of variable intensity, in some cases sufficient to annul the opposite effect, and to produce 'neutral points.' Indeed the sun is not to be regarded as the only source of atmospheric illumination, but also the air and the earth acting as reflectors. Each of these may displace the plane.

As regards the action of terrestrial magnetism, it appears that the plane of polarization undergoes rotation in a direction always the same, direct if

observed northwards, inverse if southwards. It is in the same direction as a current would take to produce terrestrial magnetism. The rotation, if due to magnetism, should not be detected in a line perpendicular to the dip needle: by experiment this is found to be the case.

The author of the memoir has already contributed valuable researches to the *Comptes Rendus* of the Academy on measuring the rotatory power of certain gases, especially of air. These have been noted in this Summary; and so has the observation of the power of terrestrial magnetism in rotating a ray passing through carbon bisulphide. By combining these with the facts above stated, M. Becquerel shows in conclusion, (1.) The existence of a variable angle between the sun's plane and that of atmospheric polarization for the same point; (2.) The periodic diurnal variation of this angle, closely connected with change of illumination; (3.) The evidence of a magnetic influence of the earth on the atmosphere, producing a small but definite deviation in the plane of polarization.

The History of Musical Pitch was given in detail by Mr. Alex. J. Ellis before the Society of Arts on March 3rd, the paper beng in some sense supplementary to a former communication on May 22nd, 1877, to which a Society's silver medal was awarded. It had been objected to the former paper that the instrument used for measuring pitch, namely, Appunn's Reed Tonometer, was hardly trustworthy, and that the results thus obtained were probably incorrect. M. Randolph Kœnig especially wrote in this sense to the *Monde* of June 19th, 1877. 'A strange and unexpected attack,' he writes, 'has been made in England on the accuracy of the French official Diapason. Mr. Ellis, having found that the notes of a Tonometer composed of sixty-five harmonium reeds did not agree with it, has thought fit to declare before the Society of Arts that the normal A does not give 870 simple vibrations but 878. Mr. Ellis having, moreover, found that tuning-forks of my manufacture were accurately in tune with the French A, did not hesitate to affirm that all these instruments, including my large Tonometer, were necessarily false.' He then quoted a letter from Helmholtz showing the inaccuracy of Appunn's standard, and intimates, with some warmth, that Mr. Ellis had neglected to verify his test instrument before using it. The writer of the present notice had already drawn the attention of the Musical Association to the same defect at a meeting on Nov. 6th, 1876. It was afterwards shown by Lord Rayleigh that the error was due to the natural influence of the reeds on one another when in a state of vigorous vibration. Mr. Ellis, in the present paper, frankly admits the charge, saying, 'What the cause of this "drawing" of the beats may be has not yet been investigated.' (This is incorrect.) 'Its direction and amount was, I believe, entirely unknown previously. I feel that I owe an apology to Herr Kœnig for having been unfortunately misled by the unknown error of Appunn's instrument to attribute that error to him. Besides this acoustical acceleration of the beats,' he continues, 'there remain two other drawbacks to Appunn's Tonometers: First, they do not retain their pitch with accuracy, and secondly, their variation with temperature is unknown. Hence they are not, as I had hoped, instruments of scientific precision, though admirable for all purposes of lecture illustrations.'

The remainder of the paper, being mainly concerned with historical

details, hardly comes within the province of Physics. It is, however, a most laborious and exhaustive discussion of a somewhat neglected subject.

Measurements in Electro-optics.—Dr. Kerr continues this important subject in the *Philosophical Magazine.* He finds that the dioptric actions of dielectrics are generally of one kind—pure double refractions with reference to the line of electric force as axis; but they vary largely in intensity, and even in sign, from one dielectric to another. Dielectrics are optically equivalent to uniaxial crystals, and exhibit like variations, both from strong to weak, and from positive to negative. Carbon disulphide is of the positive class, and in regard to strength holds a place like that of Iceland spar among crystals.

The law is most probably as follows. The intensity of electro-optic action of a given dielectric, per unit thickness, varies directly as the square of the electric force.

He describes at length a new form of cell in which a 4-inch plate of powerfully refractive and dispersive liquid is enclosed between two conductors of brass, the lower resting on its floor, the upper supported by a glass rafter. The opposed faces are flat and smooth, parallel, and distant about $\frac{1}{13}$ in. All edges and corners are rounded away. The cell is filled with about a pint of clean disulphide. Two Nicol prisms are placed in the path of a beam from outside, the ray passing between the conductors in a slit four inches long, one inch broad, and $\frac{1}{13}$ in. deep, the latter lying vertically as the line of force. Wires are led from the lower conductor to earth, and from the upper to the prime conductor. On starting the machine, the potential of the upper conductor rises slowly, and the black space between the conductors is gradually illuminated, passing from black, bluish-grey, faint white, to a pure and brilliant white. Although the highest potential reached is comparatively low, the optical effect is very large, and beyond neutralization by a band-compensator of strained glass. As the potential rises, the polariscope gives a fine progression of chromatic effects, which descend regularly and continuously through a certain range of Newton's scale. The luminous band passes from white to bright straw-colour, deepening to yellow, then through orange to deep brown, then to pure and dense red, then to purple and very deep violet, then to rich and full brown, then to green. About the last point the process generally terminates in spark discharge. The dielectric acts like a plate of quartz with optic axis parallel to the lines of force, and increasing in thickness rapidly as the potential rises.

To measure potential he uses a Thomson's long-range electrometer, and a Jamin's compensator with the axis of its prisms perpendicular to the ray, one vertical, the other therefore horizontal. A convex lens gives a distinct view of the black band and the reference-wires in the compensator. The exact process of measurement requires much detail for its description. It gave congruous results. In conclusion, the law of squares is stated in several forms. The quantity of optical effect varies: 1. Directly as the square of the resultant electric force; 2. directly as the energy of the electric field per unit of volume; 3. directly as the mutual attraction of the two conductors; 4. directly as the electric tension of the dielectric: a quantity long ago conceived clearly by Faraday, and introduced into theory by Clerk Maxwell. Their views are strongly confirmed by the new facts.

A Glycerine Barometer.—Mr. James Jordan communicates to the Royal Society the details of this instrument. Many attempts, as he states, have been made from time to time to construct barometers with fluids of lower density than mercury, with the view of increasing the range of oscillation. He expresses the belief that such instruments may show the character of more minute vibrations of atmospheric pressure at storm stations. Many have been made with water, notably one in 1830 at the Royal Society, by Prof. Daniell. These, however, are vitiated by the effects of change of temperature on the water vapour in the vacuum, which marks changes of pressure. Glycerine, from its high boiling-point, has a very low tension of vapour at ordinary temperatures, and a very small coefficient of absolute expansion. The specific gravity of the purest glycerine is 1·26, less than $\frac{1}{10}$th that of mercury. The mean height of the column is 27 ft. at the sea level; a variation of $\frac{1}{10}$ in. in the height of the mercurial column is equal to more than an inch in glycerine. As it is very hygrometric its surface is covered by a shallow layer of heavy petroleum oil.

The tube is formed of ordinary composition metal pipe of $\frac{5}{8}$ in. internal diameter. To this is cemented at the top a glass tube 4 ft. long, with inside diameter of 1 in. The upper end is formed into an open cap fitted with an indiarubber stopper. Two scales, one on either side, read off the height, one being divided into inches and tenths of absolute measure, the other into equivalent values of mercury.

The cistern is cylindrical, of copper tinned inside, 5 in. deep and 10 in. diameter, with a cover and small orifice covered with cotton wool to keep out dust. Glycerine, coloured red by aniline, was heated to 100° Fahr. and placed in the cistern; by means of an air-pump connected with the top of the tube the level was raised 323·571 inches, or within 3 of the Kew standard. A plug was then screwed in below to support the column, the tube was filled at the top with glycerine, and the stopper inserted. Some precautions were adopted to allow air to escape, and the column was finally allowed to take its own position. It will be regularly observed by the Superintendent of the Observatory.

Accidental double Refraction forms the subject of a communication to the *Annales de Chimie et de Physique* by M. Macé de Lépinay. He distinguishes four kinds: 1. By pressure or tension, discovered by Brewster. 2. Lamellar polarization, due to cubic crystalline structure. 3. Results of unequal temperature or chilling. 4. Double refraction from high electrical tension, as discovered by Kerr. The first (1) is simply proportional to the force employed, starting with a certain value, and leads to some remarkable results; for instance, quartz cut perpendicularly and compressed gives lemniscate curves, thus becoming biaxial for the time. Under the second (2) come the experiments of Seebeck in 1812 and Brewster in 1814, which have been revived with practical purpose of late by M. de Bastie in his toughened glass. These are clearly due to excessive surface tension. The writer of the memoir goes into long and careful experiments as to (*a.*) the distribution of stress in rectangular plates; (*b*) the verification of Wertheim's law of wave-lengths; (*c.*) the variations of ordinary and extraordinary indices in various points of rectangular plates. The paper itself extends to ninety pages, and contains, besides the direct objects of the inquiry, some valuable collateral

matter, such as a method of verifying Jamin's compensator with great delicacy.

Distinguishing Lights for Lighthouses forms the subject of a communication from Sir W. Thomson to the *Times*. He recommends: (1) A great quickening of all revolving lights. (2) The application of a group of dot-dash signals to every fixed light. (3) The abolition of colour as a distinction for lighthouse lights, except for showing dangers, channels and ports by red, white, and green sectors. Of about 120 revolving lights on the English, Scottish, and Irish coasts, there are in all eighteen in which the periods are ten seconds or less, and the times of extinction seven seconds or less. In these quick revolving lights, the place of the light is not practically lost during darkness; the eye, sweeping deliberately along the horizon, with or without the aid of a binocular, 'to pick up the light,' passes over less than its own field of view within the period of the light, and thus finds it almost as surely as if it were fixed. What a contrast to the ordinary minute-period revolving light!

The distinction by colour alone ought to be prohibited for all lighthouse lights, on account of its liability to be confused with ships' and steamers' side-lights. Southsea Castle, with its red and green port and starboard side-lights, seems as if actually planned to lure an unsuspecting enemy to destruction. His proposal is to distinguish every fixed light by a rapid group of two or three dot-dash eclipses; the dot of about half a second duration, the dash three times as long, with intervals of light, about half a second each, between the eclipses of the group, and of five or six seconds between groups.

Siemens's Differential Electric Lamp has one carbon attached to the end of a lever joined to a pair of iron cores, which are free to move up and down in two solenoids. One of these has large wire of small resistance, forming part of the lamp circuit. The other is a coil of smaller wire, offering greater resistance. It is in a circuit external to the lamp, joining the conductors and excluding the carbons. When the former is excited, it draws in its core, and the points of the carbons are separated; when the latter, they approach one other. The distance will thus be adjusted automatically, so as to maintain constant action.

Connection of Surveys has been recently accomplished by M. Perrier, between Algiers and Europe. He found that from all the trigonometric points of first order about Oran, the loftier peaks of Sierra Nevada were visible in clear weather. The stations chosen in Algeria were the summits of Mount Filhaoursen and Mount M'Sabiha, west of Oran; in Spain, those of [illegible] Tetica and Mulhacen—the latter the most elevated point in the [illegible]. The signals were to have been given by means of solar reflectors and powerful lenses, over a distance of 270 kilometres, but they failed utterly. Preparations had, however, been made for the employment of the electric light, and on the summit of each mountain a Gramme's machine had been established. On August 20 the lights were displayed all night. It was not until after twenty days that one after another they became visible even to the naked eye. That on Tetica, nearly 270 kilometres distant, about equalled *α* Ursæ Majoris, which rose near it. We have now trigonometric measurements of an accurate nature from lat. 61° in the Shetland Islands, to lat. 34° on the southern frontier of Algeria.

A New Standard of Light has been suggested by Mr. Louis Schwendler, in the *Journal* of the Society of Bengal. It consists of a U-shaped piece of platinum foil, about 20 mm. in length, each limb being 3 mm. in breadth, and at the top with metal clips: a current, the amount of which is registered by a galvanometer in circuit, is passed through this. It is a step in advance of the candle standard, though not of the same accuracy as other scientific units.

Coloured Rings on the Surface of Mercury have been obtained by M. Guebhard, by clearing it of oxide and breathing on it. They contract as evaporation diminishes the thickness. The best results are obtained with collodion, diluted by means of ether. These pellicles can be detached and transferred to paper.

ZOOLOGY.

A Synthetic Starfish.—Under the name of *Astrophiura permira*, Mr. W. Percy Sladen has described (*Annals and Mag. Nat. Hist.* December, 1879) a most remarkable form of Echinoderm from the coast of Madagascar. While the ordinary starfishes present usually the well-known star-like form, with five or more rays springing from a central body with which they are perfectly continuous, the body in the Ophiurids is a rounded or more or less pentagonal disc, from which issue five jointed arms, quite distinct in structure from the disc and from the much stouter rays of the ordinary starfishes. Mr. Sladen's new form combines the characters of the two groups in a very singular manner; and curiously enough, it is towards the somewhat aberrant forms of starfishes (such as *Goniodiscus*) in which the enlargement of the disc and shortening of the rays converts the whole body into a pentagonal disc, that the new type seems most to approximate in outward appearance. In fact, the arms are for the greater part of their length enclosed in a disc formed of calcareous plates both above and below, but a small portion of jointed arm projects from each angle of the pentagon thus formed, and with the structure displayed along the lines of the arms on the lower surface sufficiently demonstrates the Ophiuridan affinities of the organism. By careful study indeed Mr. Sladen makes out that the whole skeletal structure is due to an abnormal development of the ordinary plates of an Ophiurid; but at the same time he recognizes in the structure of the animal a number of characters which tend towards the Asteroida, such as a great development of the ambulacral system, with formation of supplementary plates separating the tentacular compartments, the extension of the peritoneal cavity into the radial portions of the animal, and the organization of the mouth. Mr. Sladen's paper, which is illustrated with an excellent plate, deserves the attention of all zoologists.

FEATHER-STARS, RECENT AND FOSSIL.

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[PLATES V. AND VI.]

THE *Comatulæ*, or Feather-Stars, have long been known as among the most beautiful and interesting of the inhabitants of the shallow water round our coasts. They are found in all parts of the world, and at all depths down to 2900 fathoms. The Arctic Expedition of 1875-76 dredged two species in Kennedy Channel, above 80° N. Lat. One at least of these is common in the seas of Spitzbergen and Nova Zembla, while a closely allied form was obtained by the *Challenger* at Heard Island in the Southern Sea. They are largest and most varied in the tropics, especially in the shallow water about the Philippine Islands and in the Malay Archipelago. At great depths they are much dwarfed, and at the same time somewhat rare, the *Challenger* having met with them but seven times at depths exceeding 1000 fathoms.

The *Comatulæ* belong to the *Crinoidea*, which is a very well-defined class of the great sub-kingdom ECHINODERMATA, differing from the other members of the group, *e. g.*, the Starfishes, Sea-Urchins, and Sea-Cucumbers, by various peculiarities.*

While the Starfishes and Urchins crawl about mouth downwards on the sea-bed, by the aid of numerous sucking feet, the Crinoids remain more or less fixed in one spot, lying on their backs or growing on stalks, with the mouth upwards. Their arms which may be from five to about two hundred in number, are fringed by small appendages, the pinnules (Pl. VI. figs. 1, 2, *pi.*), which alternate on opposite sides of each arm, and give rise to the appearance which is denoted by the name Feather-Star. The arms may be more or less completely extended, or one or more of them may be curved inwards over the mouth; but

* Compare 'Notes on the Ophiurans, or the Sand and Brittle Stars,' by Prof. Duncan, F.R.S., *Pop. Sci. Review*, N. S. vol. ii. (1878) pp. 339, 342.

they have never been seen to take part in the prehension of food. It is, however, indirectly obtained by means of the arms.

On the upper surface of each arm and pinnule there is (in most cases) a narrow groove (Pl. V. fig. 2, *f.g.*, and Pl. VI. fig. 1, *f.g.*), which is lined by a number of those delicate little protoplasmic filaments, known to naturalists as cilia. These are in a state of continual vibratory movement, which is always in the same direction, viz. towards the mouth. Currents are thus set up in these food-grooves, all making for the mouth, to which the grooves converge. Any of the small alimentary particles scattered in the water which may happen to settle down in one of these food-grooves are thus carried along it towards the mouth. The grooves of adjacent arms unite in succession so as to form from five to ten primary groove-trunks, which end in a peristomial area of variable size and shape. This may be either almost in the centre of the body (Pl. V. fig. 1 and Pl. VI. fig. 12), or altogether eccentric (Pl. VI. fig. 11). Within this peristomial area is a narrow slit, the mouth (*m*), into which are poured the contents of the various food-grooves. This, though mostly microscopic, appears to vary very much in character, according to circumstances. Dr. Carpenter has found the stomach of the Arran *Comatulæ* to contain large quantities of the horny remains of infusorial animalcules, while in other specimens the horny casings of Entomostraca, or of the larvæ of higher Crustacea, have been found in the stomach. I have myself removed from the stomach of a large tropical Comatula the body of an Isopod Crustacean nearly half an inch long, while in another case I have found the food-grooves to contain numerous foraminiferal shells, *Rotaliæ*, *Biloculinæ*, and others.

The whole of the coiled digestive tube is lodged in the body of the *Comatula*, no part of it extending into the arms (Pl. V. fig. 1, *d.*) This body consists of two parts, (1) the cup or calyx formed by the skeleton, and (2) the visceral mass, usually termed the disc, which is supported in this cup, but is sometimes very readily detached from it, as has been the case with the discs represented in Pl. VI. figs. 11, 12. The disc is usually more or less hemispherical in shape (Pl. V. fig. 1), the lower convex portion fitting into the cup, while on the flattened upper surface (Pl. VI. figs. 11, 12) is the mouth, with the food-grooves converging towards it, and a tubular projection, at the end of which is the second opening of the digestive canal.* When the mouth is central, as in the genus *Antedon* (Pl. VI. fig. 12), the anal tube (*a.t.*) is in one of the interradial spaces on the disc between two of the food-grooves; but when the mouth is eccentric, as in

* I have one monstrosity, an *Actinometra*, with two mouths and two anal tubes.

Actinometra (Pl. VI. fig. 11), the anal tube is nearly or quite central.

In our English *Comatulæ* the disc is almost or entirely bare; but in many tropical forms it is covered by a very complete mosaic pavement of closely fitting plates (Pl. VI. fig. 12), which occupy all the spaces between the food-grooves, and even extend out for some distance on to the upper surface of the arms and pinnules at the sides of their median grooves.* Most of the plates nearest the grooves are pierced by minute holes (*w.p.*), the 'water-pores.' These are the upper openings of tiny funnel-shaped tubes, which open below into the body cavity, and are lined by cilia all working inwards, so that the body cavity is in free communication with the external water. These pores are likewise present in *Comatulæ* with naked discs (Pl. V. fig. 1. *w.p.*); and they have also been found on the lowest parts of the arms, and even piercing the plating of the pinnules, where they lead into a tubular extension of the body cavity into the arm, containing the generative gland.

On the naked discs (Pl. VI. fig. 11) each side of the food-groove is formed of an elevated fold of skin scolloped at its edge so as to form a row of minute triangular leaflets. At the base of each of these is a group of three delicate tubular tentacles, one of which is much longer than the other two. The leaflets alternate on opposite sides of the groove, and are ordinarily erected, with the tentacles projecting considerably beyond them. But the tentacles may be withdrawn and the two folds closed down, the leaflets meeting in a sinuous line, so as completely to cover the furrow. The same is the case with the grooves of the arms and of the pinnules borne by them, as is seen in Pl. VI. fig. 1, while some arms are occasionally altogether ungrooved. (Pl. VI. fig. 2.)

In the plated tropical *Comatulæ* matters are much more complicated (Pl. VI. fig. 10.) The edges of the groove are supported by a series of more or less oblong side-plates (*adambulacral*), and hinged to the upper side of each of these is an oval covering-plate† (*superambulacral*). These may be erected and the tentacles extended, or they may be closed down so as completely

* The ventral plating of the disc is so very complete in some recent *Comatulæ*, that no mouth is visible at all in the dry state. The summit-plates of many Palæozoic Crinoids, such as *Cyathocrinus*, and also of the Blastoids appear to me to be strictly comparable to those of these recent forms. Hambach, however, speaks of the summit-plates of the Blastoids as non-existent or rather as proving on close examination, to be Bryozoa, or ovulum-like bodies!—*Trans. St. Louis Acad. Science*, vol. iv. No. 1, p. 150.

† Some recent Crinoids have covering-plates only, and no side-plates. The same variability occurs in the Palæozoic Crinoids, and perhaps also in Blastoids. Some species have both series represented, though, of course, in a greatly reduced form, while others have the covering-plates only. Hambach's description of the zigzag-plated integument of the Blastoids, as 'probably of an elastic texture during the lifetime of the animal,' professedly

to cover the groove, the succeeding ones from opposite sides overlapping each other, while the tentacles are retracted. The holes, (*s*) represented in fig. 10 between the adjacent side-plates mark the positions of the dark spots or 'sacculi,' which occur at the sides of the food-grooves of nearly every *Antedon*, and are also to be found in the interior of the body. (Pl. V. fig. 1. *s*.)

These tentacles of the Feather-stars correspond to the tube-feet of a Starfish-arm, and contain side branches of a tube which is situated in the middle line of the arm beneath the food-groove, and is known as the water-vessel, or ambulacral vessel (Pl. V. figs. 1, 2, *w.v.*) All the water-vessels of the different arms unite in succession as the food-grooves do, and converge to join the water-vascular ring, an annular tube, situated in the lip around the mouth.* From this ring a number of delicate tubules—the 'water-tubes' (*w. t.*)—hang down into the body cavity, into which they open. It contains water which has entered it by the water-pores on the disc, so that the ambulacral system is indirectly in communication with the water in which the animal lives. We are not yet quite clear about the real nature of the water-vascular system of the Echinoderms; but it seems tolerably certain that it forms an important part of the breathing apparatus of the Crinoids, and that it contains some oxygen-carrying substance which is able, in the thin-walled tentacles, to exchange carbonic acid for the oxygen of the surrounding water.

Superficial to the water-vascular tube of each arm is a smaller tube, the radial blood-vessel (*b. r.*) which is connected like its fellows with a blood-vascular ring around the mouth. Situated in the vertical axis of the disc is a lobulated organ known as the central plexus, from its consisting of a bundle of blood-vessels. (Pl. V. fig. 1, *c.p.*) Some of these terminate above in the oral blood-vascular ring, while others extend outwards into the rays and arms and surround the genital glands. Others again give off side branches which form a network over the digestive tube. Towards the bottom of the visceral disc the vessels of the central plexus group themselves into an inner set, surrounded by five outer ones, and so descend into the calyx. They pass through the central funnel between the inner ends of the first radials, at the bottom of which the five outer vessels corresponding in position with the radials expand considerably so as to form the 'chambered organ.' This consists of a central axis of minute vessels with five chambers (Pl. V. fig. 1, *ch.*) clustered round it like the carpels of an orange. It is contained in a small, more or less basin-shaped plate, known as the centrodorsal piece. (Pl. V. fig. 1.

refers to these plates. What he does describe, however, is something entirely different.

* Compare Duncan, *loc. cit.* pp. 347–351.

Pl. VI. figs. 7, 9, *cd.*) This forms the dorsal or lower pole of the body and bears on its exterior a number of jointed appendages, the cirri (*ci.*) by which the animal anchors itself. Each cirrus is pierced by a central canal lodging a blood-vessel (*c. v.*) which is continuous through the wall of the centrodorsal with one of the chambers of the chambered organ, or with one of the vessels in its central axis.

Soldered on to the centrodorsal are the five first radials (Pl. V. fig. 1, Pl. VI. figs. 7, 9, r_1.) forming the lower part of the cup in which the disc rests. Jointed on to them and attached to them by muscles are the five second radials (r_2.). Each of these in its turn bears a third or axillary radial (r_3.), the outer face of which is not flat but roof-shaped, and usually bears the lowest joints (br_1) of two arms working on it by means of muscles, as the second radials work on the first. These arms, which are thus primarily ten in number, consist of a series of joints that may be all like the first. But in other cases some of the joints may be axillary, so that the primary arms fork as the rays do. In the deep-sea *Comatulæ*, in the two British species, and in those from cold climates, the ten primary arms rarely divide, but in the tropical species the forking of the rays may be so often repeated that the number of arms becomes very great, sometimes reaching nearly two hundred. Each arm-joint (with a few exceptions) bears a similarly jointed appendage, the pinnule (Pl. VI. figs. 1, 2, *pi*), which is merely a small edition of the arm, containing prolongations of the water-vessel, the blood-vessel, the body-cavity, and in the case of the lower pinnules of the arm, the genital glands also (Pl. V. fig. 2).

Each joint of the rays, arms, and pinnules, is pierced by a central canal lodging a fibrillar cord (Pl. VI. figs. 1, 2, *a. c.*), that proceeds through the first radials from a yellowish fibrillar envelope (*sh.*) around the chambered organ. A sheath of the same substance surrounds each of the cirrus vessels (*c. v.*) that proceed downwards and outwards from the chambered organ.

From each of the interradial angles of the chambered organ five large cords pass upwards and outwards, and fork almost immediately. (Pl. V. figs. 1, 3.) The right branch of one fork and the left branch of its neighbour enter two adjacent openings on the inner face of each first radial. They run side by side through its central canal and on into the third radial where each of them forks. The two right branches enter the central canal of the skeleton of the right arm, while the left branches enter that of the left arm to form their respective axial cords. Before leaving the third radial, however, these two cords are united by a transverse commissure. There are also commissures in the first radials. The two cords which each contains are united with one another and with those of adjacent radials by

one continuous circular commissure lodged like the other cords in special canals.

What is the meaning of this excessively complicated arrangement? Experiment shows that the movements of the arms are dependent upon the integrity of their axial cords and upon the connection of these cords with the central fibrillar envelope of the chambered organ. The swimming movements of a Feather-Star are exceedingly active, and are also performed with a singular regularity. When a ten-armed animal swims, all the five right arms are simultaneously bent, and then the five left arms. As long as the swimming lasts this alternating movement is kept up with the most perfect regularity. Owing to the length of the arms and to the small size of their component joints, of which there are frequently more than one hundred to each arm, the number of muscles concerned in the movement reaches at least one thousand pairs.

Experiment shows that these muscles are under the influence of a governing centre, which not only regulates their contractions, but co-ordinates these contractions in the most remarkable manner. This co-ordination is well shown in the following experiment. When one of the first pair of pinnules on the arm is irritated, the whole circlet of arms is suddenly and simultaneously closed over the disc; but irritation of one of the ordinary pinnules higher up the arm is simply followed by flexion of the arm which bears it. The governing centre on which this action depends has been shown to be situated in the fibrillar sheath of the chambered organ; and the axial cords of the rays and arms are the channels by which the influence of the centre is communicated to the muscles. For the swimming movements of the whole animal depend upon the integrity of the chambered organ, and may take place after the visceral disc has been removed from the skeleton, which contains the chambered organ and axial cords, and swims about on its own account. The swimming movements are therefore entirely independent of any structures contained in the disc. In the same way the movements of each individual arm depend upon the integrity of the axial cord of that arm, stopping directly it is injured; and microscopic investigation shows that branches of the axial cord are distributed upon the ends of the muscular bundles which connect the successive joints of the rays, arms, and sometimes also of the pinnules.

The above facts seem to show that the axial cords of the rays and arms, together with the fibrillar sheath of the chambered organ in which they originate, constitute a system of motor nerves of no little complexity. The only difficulty in the way of this view is that nothing of the kind is known in the other Echinoderms. The Star-fishes, for example, have a fibrillar band between the radial blood-vessel and the cellular lining of

the median groove of each arm, which is connected with an oral ring. This ring, and the bands connected with it, constitute a nervous system situated on the ventral side of the body.* A similar oral ring with radial prolongations occurs also in the Feather-Stars (Pl. V. figs. 1, 2, *n. r.*). But it is altogether unconnected with the muscles, and has no influence whatever on the swimming movements. After the visceral mass has been turned out of the calyx altogether these movements go on just as well as before, while the movements of any arm are entirely independent of the ventral nerve of that arm; for the ungrooved arms (Pl. VI. fig. 2), with no ventral nerve, swim just as well as the grooved arms with a ventral nerve, even after the latter has been divided. In some tropical *Comatulæ*, half, or more than half, of the arms are in this ungrooved condition.

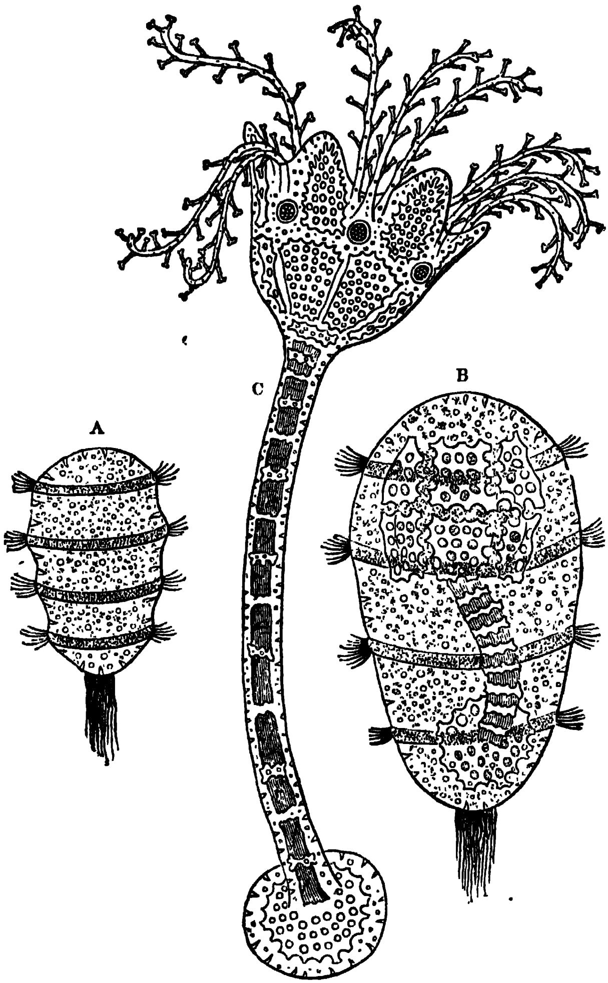
While on the one hand the nervous nature of the ventral fibrillar band can hardly be denied, owing to its structural identity with the nerves of other Echinoderms, we must admit, on the other hand, that the very considerable locomotive powers of the *Comatulæ* are localized in the chambered organ and its connections. These are altogether unrepresented in the other Echinoderms, but in spite of this, and of the singularity of their position, we cannot, with justice, refuse to consider them as nerves. Some day, perhaps, their presence will be understood.

Between the dorsal skeleton of the arms and pinnules, and the water-vessels on their ventral side, are three tubular prolongations of the body-cavity. The middle one is known as the genital canal (Pl. V. figs. 1, 2, *g, c.*), because it contains the genital gland (*ov.*) surrounded by a blood-vascular space, which is connected with the vessels of the central plexus. The canals above and below this one, are known respectively as the ventral or sub-tentacular (*st. c.*) and the dorsal or cœliac (*c. c.*). They communicate with one another at the end of each arm or pinnule, and are connected in the body with different parts of the body-cavity, which is full of water containing the products of digestion. The sub-tentacular canal arises from a large central space (*a. b.*) in the axis of the body, around which the digestive canal is coiled, and a current proceeds through it to the tip of each arm and pinnule, which returns to the body by the cœliac canal. This current is produced by the vibration of cilia, which are not uniformly distributed, but localized in little cups on the top of each pinnule-joint (Pl. V. fig. 2, *ci. c.*).

The young of the Feather-Star leaves the egg as a little oval body about $\frac{1}{50}''$ in length, shaped somewhat like a small barrel, and surrounded by four hoops of long vibratile cilia with a still longer tuft of them at its hinder end (woodcut A). By means of these cilia it swims about in the water. After a while, slender limestone rods make their appearance near the front

* Compare Duncan, *loc. cit.* pp. 348–351.

end, and by repeated forking and joining these rods give rise to ten plates of a delicate calcareous network, which are arranged in two cross rings of five plates each (woodcut B). Passing back-



Three stages in the development of *Antedon rosacea* (after Sir Wyville Thomson).* A. Larva just hatched; B. Larva with rudiments of the stem-joints; C. Pentacrinoid larva.

wards from beneath the centre of the lower ring of plates is a series of delicate calcareous rings, which become supported, later on, by bundles of longitudinal rods forming inside them. The last

* We are indebted to the courtesy of Sir John Lubbock for permission to use this woodcut.

of these bundles, at the hinder end of the larva, rests against a circular plate of considerable relative size. At this stage the larva has the form of a bent club or rod with an enlarged head. The ciliated bands disappear, and it gradually loses its power of swimming, attaching itself to some stone or other solid substance by its base, the knob of the club being free (woodcut c). This knob gradually becomes the body of the future *Comatula;* while the series of rings between it and the base of attachment make up a short supporting stem. This is known as the Pentacrinoid stage of development, owing to the resemblance between it and another Crinoid (*Pentacrinus*) which remains stalked throughout life, while the larval *Comatula* is only so for a time. For it eventually separates itself from all but the top joint of its stem, and anchors itself by the cirri that are developed upon this top joint, which developes into the permanent centrodorsal piece, being rather larger than the other stem-joints from the very first. The ring of plates resting upon it are the basals, and the plates above them, the orals. The enlarged head gradually becomes five-lobed, each lobe answering to an oral plate; and after a while these plates separate like the petals of a flower so as to expose in the centre (*Antedon*) the opening of the permanent mouth. Around it are five groups of three tentacles each alternating with the oral-lobes. At this early stage no radials are present, but only the basal-plates resting upon the centrodorsal piece beneath, and supporting the orals above. The space between these two rings gradually increases, the orals being carried away from the basals as the calyx widens out. The first radials appear in this intervening space between the basals and orals, alternating in position with both; and in correspondence with them the rays grow out as rapidly-elongating processes, in which the other radials are successively developed, so that the diameter of the calyx increases fast. The oral plates retain their primitive relation to the mouth, and so get carried further and further away from the basals, while the first cirri begin to appear on the centrodorsal ring that supports the basals. The orals gradually become absorbed, and soon after the head detaches itself from the stem and commences life as a free *Comatula* they disappear altogether. At the same time the basals undergo a very singular series of changes which result in their being no longer visible on the exterior of the calyx.

Very soon after the young *Comatula* is detached from its stem, the centrodorsal begins to extend itself over the dorsal surface of the ring of basals so as finally to conceal it altogether. At the same time the shape of these plates becomes altered by removal of one part and the deposition of new calcareous matter at another part. The original ring of five pentagonal plates thus undergoes transformation into the 'rosette,' a small ten-

rayed circular plate (Pl. VI. fig. 8) which rests on the middle of the upper surface of the centrodorsal piece so as to cover in the chambered organ above. (Pl. V. fig. 1, *ro*.) It is lodged in the lower part of the funnel formed by the first radials, which now rest directly on the centrodorsal, and it becomes more or less closely united to their inner ends, so that when the circlet of radials is separated from the centrodorsal, the rosette comes away with it. This is shown in Pl. VI. fig. 6, representing the under surface of the radials of *Antedon celtica* with the rosette (*ro*) in the centre of the ring formed by them.

This metamorphosis of the basals of recent *Comatulæ* into a concealed rosette is a very singular circumstance, because it shows that the *Comatulæ* have, as it were, taken a new departure, and have branched off from the type of *Pentacrinus* in the resemblance of which they have developed. In all the recent *Pentacrinus* species and in most of the fossil ones the radials are cut off from the top stem-joint by a more or less complete ring of basals. (Pl. VI. fig. 3, *b*.) These basals are sometimes small, and only just in contact by their central ends, while their outer ends appear externally as small buttons separating the radials from the top stem-joint at the angles of the calyx. There is every gradation between this condition and that in which they meet one another all round, so that the radials rest entirely upon them, and are nowhere in contact with the stem-joint; but we know of no recent *Pentacrinus* in which the basals do not appear externally, though some of the fossil forms are in this condition. On the other hand, we have no certain knowledge of any fossil *Comatulæ* with a rosette. The earliest known *Comatulæ* occur in the Inferior Oolite of this country. Figs. 4 and 5 on Pl. VI. are two views of the calyx of one of them, *Ant. cheltonensis*. The difference between them and figs. 6 and 7, which represent the corresponding parts of a recent species, is at once apparent. In the fossil the basals form a five-pointed star, lying beneath the under-surface of the radials (fig. 5); while in the recent species all that remains of them is the inconspicuous rosette occupying the gap between the inner ends of the radials (fig. 6).

The Oolitic rocks of South Germany and of Switzerland, which are of approximately the same geological age as the Oxford Clay and Coralline Oolite of the British area, contain several different kinds of *Comatulæ*, far more than are known in this country. Fig. 9 on Pl. VI. represents the calyx of one of them, *Ant. scrobiculata*.

All the Oolitic *Comatulæ* have basal pieces like those of this species and of *Ant. cheltonensis* (figs. 4, 5), though they are not always so large. In some forms they do not appear at all on the outside of the calyx, so that they are only visible in specimens from which the centrodorsal has been removed. This is

especially marked in the Cretaceous species; for while some Oolitic forms have really large external basals, this is not the case in any Cretaceous species, while they are occasionally absent altogether. As, however, we are unable in these cases to see the interior of the calyx, it is impossible to determine whether a rosette is present or not. It is the same with the only two Tertiary species the calices of which are known to us.

We cannot, therefore, say with certainty when the *Comatulæ* first ceased altogether to retain their larval basals on the exterior of the calyx, as *Pentacrinus* species do still (though some of them formerly did not), and as most of the Oolitic *Comatulæ* did.

It is certain, however, that the final change occurred after the middle of the Cretaceous period; for *Antedon Lundgreni*, from the Upper Chalk, had *Pentacrinus*-like basals appearing externally. The disappearance of the basals from the exterior of the calyx was not completed, therefore, even at this comparatively recent date; and it is just possible that a very remarkable species, now living (?) in the Indian Ocean, may be the last survivor of this more generalized form of *Comatula*, with *Pentacrinus*-like basals and no rosette. Unfortunately but one example of this species has ever been discovered. Fifty years ago it was dissected, and a description was written of it; but it is not complete enough to enable us to settle this interesting question. At any rate, none of the deep-sea *Comatulæ* brought home by the *Challenger* have any resemblance to the older species, every one of them having a rosette.

Other questions which naturally arise are the following: For what reason did the basals of *Comatulæ* cease to be five more or less separate pieces and undergo transformation into the rosette; and why is it that *Pentacrinus* and *Comatula* have varied in different directions? All recent *Pentacrinus* species have external basals, but some fossils have not. Some fossil *Comatulæ* have them and others have not; but they do not appear in any recent forms (with one possible exception), for the larval basals never become prisms or wedges the outer ends of which remain external like those of *Pentacrinus*, but they disappear altogether into the interior of the calyx. These questions must remain unanswered, at any rate, for the present, if not permanently.

It may be pointed out, in conclusion, that in the class Crinoidea, as in so many other groups of animals, the recent forms are the most highly specialized; and the further we go back in geological time, the more nearly (on the whole) do the *Comatulæ* approach *Pentacrinus* and other stalked Crinoids to which they have so much resemblance in their early stages of development.

DESCRIPTION OF THE PLATES.

The following letters denote the same parts throughout all the plates.

a. b. Axial body cavity. *a. c.* Axial cords of the skeleton. *a. t.* Anal tube. *b.* Basals. br_1, br_2, br_3, &c. First, second, and third brachials, &c. *bv.* Radial blood-vessel. *c. c.* Cœliac canal. *cd.* Centrodorsal piece. *ch.* Chambered organ. *ci.* Cirrus. *ci. c.* Ciliated cups. *c. p.* Central plexus. *c. pl.* Covering-plates of food-groove. *c. v.* Cirrus-vessel. *d.* Digestive tube. *e.* Epithelial lining of food-groove. *f. g.* Food-groove. *g. c.* Genital canal. *m.* Mouth. *n. v.* Ventral nerve. *ov.* Ovary. *pi.* Pinnule. *p. j.* Pinnule-joint. r_1, r_2, r_3. First, second, and third radials. *ro.* Rosette. *s.* Sacculi. *sh.* Fibrillar sheath of chambered organ. *s. pl.* Side-plates of food-groove. *st. c.* Sub-tentacular canal. *t.* Tentacle. *w. p.* Water-pores. *w. t.* Water-tubes. *w. v.* Water-vessel.

PLATE V.

The three figures on this plate are copied, with slight variations, from Dr. H. Ludwig's 'Beiträge zur Anatomie der Crinoideen,' Morphologische Studien an Echinodermen, *Vol. I. Leipzig,* 1877–1879.

FIG. 1. Diagrammatic vertical section through the body of *Ant. rosacea.* On the right side the section passes along a ray, and on the left side it is interradial.

Explanation of the shading: black, nervous system; red, blood-vascular system; oblique shading, water-vascular system.

FIG. 2. Cross section of a pinnule of a sexually mature female *Ant. Eschrichtii.* × 50.

The exigencies of space have necessitated the reversal of this figure. The skeleton should be downwards, and the food-grove upwards, as in fig. 1.

FIG. 3. Diagram of the distribution of the axial cords within the calyx of *Comatula*, showing their origin in the fibrillar envelope of the chambered organ.

PLATE VI.

Figures 1, 2, 10 *and* 12, *by permission of the Lords Commissioners of the Treasury.*

FIG. 1. Piece of a grooved arm of a new *Actinometra* from the Philippines, seen from above. × 4.

FIG. 2. Piece of an ungrooved arm of the same specimen. × 4.

FIG. 3. Calyx of a recent *Pentacrinus*, from the side. × 4.

FIGS. 4 and 5. Side and under views of the calyx of *Antedon cheltonensis* from the Inferior Oolite of Cheltenham. × 4.

FIG. 6. Under view of the calyx of *Ant. celtica*, showing the rosette (*ro*) in the centre of the ring of first radials (r_1). × 7.

FIG. 7. Side view of the united calyx and centrodorsal piece of the same species. × 7.

FIG. 8. Rosette of *Ant. rosacea*, seen from above. × 15.

FIG. 9. Calyx of *Ant. scrobiculata*, from the White Jura of Württemberg. Copied from Goldfüss.

FIG. 10. Side view of three joints of a pinnule of a new *Antedon* from the Pacific, showing the side-plates (*s. pl.*) bounding the food-groove, and the covering-plates (*c. pl.*) which rest upon them. The dark spots between every two side-plates indicate the positions of the sacculi. × 20.

FIG. 11. Disc of *Act. solaris*, with open food-grooves. Seen from above. × 5.

FIG. 12. Plated disc of a new *Antedon* from the Pacific, with the food-grooves converted into tunnels by the folding down of the covering-plates at their sides. × $5\frac{1}{2}$.

THE PORTLAND BUILDING STONE.

By the Rev. J. F. BLAKE, M.A., F.G.S.

FROM a builder's point of view the name of Portland Stone has a very different meaning from that it would have in the mouth of a geologist. To the former it would signify the produce of certain quarries, whence the stone employed in building St. Paul's Cathedral and other edifices was obtained; while to the latter it would indicate the produce in the way of limestones more or less suitable to building of a certain period of the earth's history. The first is much the most restricted meaning; for, however great similarity geologists may find in the same formation when traced over wide areas, for practical purposes, such as the discovery of valuable building stone, it is well known, or should be, that the minor changes in a bed take place very rapidly: so that it is a rare circumstance to find stone, though on exactly the same geological horizon, having the same value in quarries a few miles apart. Ignorance of this fact is said to have been the cause of the failure in the selection of good samples of dolomite for the construction of the Houses of Parliament; the stone of one quarry was recommended, and the substitution of stone from another quarry was thought to be immaterial, the stone being geologically the same.

In the case of the Portland Stone, though so-called Portland rocks are quarried in many places and used for building, in not one of them, except at a quarry near Tisbury, is the workable stone on the same geological horizon or of the same quality as that which is so famous. By an accurate survey of all the localities in which Portland rocks have been worked, the details of which have recently been laid before the Geological Society, I am enabled to reconstruct, to a certain extent, the physical geography of the period, and to assign the various beds of workable stone to their proper position in the series.

During the latter part of that portion of time known on the Continent as the Upper Jurassic Period, in which our so-called Middle and Upper Oolites were formed, the greater part of what is now France and England was submerged beneath an open sea, into whose depths fell gradually the fine argillaceous deposits of the Oxford and Kimmeridge Clays. These were doubtless derived in large measure from the denudation of the Lias, which spread in former times far to the west of its present boundary, and whose worn edges were covered in Dorsetshire and Devonshire by the deposits of the next subsequent submergence, the Greensand and Chalk. This open Jurassic sea did not retain an unbroken tranquillity; again and again was its floor upheaved, or other disturbances took place which left their marks in the changed nature of the rocks, and these changes were not continuous over the whole area, but were limited now to one locality and now to another. Nor did they take place approximately at the same time; for while the clay was still being deposited in one place, we learn both from actually tracing the rocks and by examining their fossil contents that limestone or sand were being formed at another. To these local changes and their resulting deposits the name of episodes has been assigned; and thus we may speak of the Portland rocks as representing the later of the two episodes which in our own country affected the upper Jurassic seas. It has been the custom amongst Continental geologists to call by the same name the later of two episodes that have affected their particular country; that is, the upper mass of limestone found in the midst of their clays has been called Portland. These, however, have nothing in common with our own except the order in which they have occurred. They are all of greater antiquity; and nowhere out of England are true Portland limestones found except at Boulogne and, as is reported, in the Pays de Bray.

In the general rise of the earth's surface which closed for us the Jurassic period, the area surrounding the English Channel appears to have remained longest below the sea, and to have contained the last deposits of that era. There is something peculiar, therefore, about the circumstances of the deposition of our Portland limestone which is of interest in itself. Moreover, of all the beds which pass by that name in England amongst geologists, that which has been celebrated amongst builders is the youngest.

This will be seen by a brief account of the 'Portland Stone' as a building stone, as it is worked in the various parts of the country where it occurs, beginning at the north, and ending at the typical locality in the extreme south. There is no great thickness of these rocks at their most northerly localities,

though they cannot be said to die out. Numerous quarries are opened in them along a range of country about six miles broad, which extends from a little north of Aylesbury to a line between Brill and Thame. The best quarries are on the road between the latter town and Aylesbury; those at Stone having been worked over two hundred years, and many good buildings erected from them. There are two main blocks, the upper a softer and more uniform stone, and the lower hard, irregular, and fossiliferous, with a bed full of Trigonias at its base. In the neighbourhood of Brill a particular bed, perhaps even lower in the series, is a rather more sandy limestone of darker colour, known as the 'greys.' None of these are oolitic, or of such super-excellent quality as would ever draw general attention to them. Though of no great thickness or importance, they are the only representatives of the Portland Stone in this district, for immediately above lie thin-bedded, white, freshwater limestones usually assigned to the Purbeck series. It is interesting to see the rapid change from one condition to the other without any mark of disturbance, and difficult to conceive the exact set of circumstances that should have rendered it possible; yet occasionally one does get a glimpse of the intervening conditions, as when in a quarry is seen a great rounded excavation, in shape like the section of a river, filled with the mud of Purbeck times, and then buried and preserved by overlying deposits. In such a case one sees the very river which emptied itself elsewhere into the lake where cyprids abounded, pond-snails flourished, and insects dying dropped their tiny wings. In places, too, we even find the boundary of the area of deposit, or at least its neighbourhood, where all things seem reduced to a minimum, and formations elsewhere important lie in a few feet. Thus in a quarry at Long Crendon may be seen in not more than eight feet four formations. Below is the Portland Stone, here worked for rough building material, then a foot or two of Purbeck-limestone beds broken at the top, then the ferruginous beds of the Lower Greensand, and over all about two feet of Gault with characteristic fossils! No wonder in such a neighbourhood the Portland rock should be somewhat deficient.

The worked stone of Oxfordshire is but a continuation of the same bed in Bucks, and is of no great value. Nevertheless, at Great Hazeley are beds which have long been worked, and are of a more solid quality; at the base, and also above the best block, is a bed full of the characteristic Trigonias.

The next district in which Portland Stone is worked is near Swindon, in north Wiltshire. The same beds which we have already seen yielding an inferior kind of stone are worked for local purposes in small quarries in the neighbourhood of that

town, and in a small outlier at Bourton. But the great quarries at Swindon itself, yielding a stone of considerable importance and which has been very largely worked for houses and churches in the district, belong to a higher part of the series; in fact, they are seen overlying the other. The stone here occurs in a very curious manner. There is a considerable thickness of soft material, consisting partly of loose calcareous sands and partly of comminuted shells, with occasionally a layer of decomposed but not broken shells. In this mass are great blocks of irregular shape, lying often in the direction of some falsebedding rather than horizontally. These are indurated by so great an amount of calcareous matter, that where the sand was originally prevailing they are hard and gritty; where the shell fragments were once the main constituent they become excellent building stone; but from their irregularity, working them must be somewhat precarious. In this quarry a geologist cannot fail to be struck with the singular complexity of the succeeding deposits, which, as will be seen, must represent in time the period when the far-famed stone of the Isle of Portland might have been forming. One can see represented as in a picture many of the features of that ancient land, on which but little further to the south flourished the cycads and conifers of the Purbeck. Here one may almost see the petrifying spring issuing from the underlying limestones, by its consolidated deposits of calc-tuff; there one may trace the winding course of the ancient river, with its stone-covered base and muddy banks, its channel now filled with carbonaceous clay. Here is represented the tranquil lake, in which the calcareous matter sank to rest or was precipitated; and there, perhaps, the remnant of the soil which once covered the dry land, but was redistributed on its submergence. The teachings of this quarry are most instructive and interesting, though one unversed in field geology might need some guide, philosopher, or friend, to read them for him aright.

Out of the Isle of Portland itself, the best stone is that obtained from the Vale of Wardour, through which the South Western Railway runs between Salisbury and Templecombe. There are many large quarries about Tisbury which send stone away far out of the district, though less appears to be worked there now than there formerly was. This stone is on the same geological horizon as that which is found in the great quarry at Swindon, and with one exception all the quarries are excavated for this. It is, however, of a very superior quality. It is scarcely a limestone, but rather a calcareous freestone, for it is composed of very fine grains of sand consolidated by calcareous matter. It is sometimes obscurely false-bedded, and

comparatively free from joints. The quarries present perpendicular cut faces, retaining the marks of the instruments used for the extraction of the stone. Blocks of considerable size are extracted, and easily cut while soft into any required pattern; the colour, however, is not very good in the fresh state, being a yellowish-grey, but soon tones down on exposure to the weather into a nondescript 'stone' colour; and in this respect, therefore, the stone is inferior to the production of the island. In some quarries the goodness of the stone is interfered with, though its interest for geologists is increased by a band of chalcedony, which has congregated sometimes round Trigonias and sometimes round corals. The latter are very beautiful objects, of which specimens are scattered through many collections; but they are now apparently utterly exhausted, and no more are to be obtained. These bands have doubtless, like the flints, been formed by infiltration through the surrounding mass, but here lie in bands instead of irregular nodules, because of the more regular stratification of the rock in which they occur. Mentioning these, however, it may be added that overlying these very building stones and forming their 'bearing' is a mass of white, calcareous, soft rock with siliceous nodules, so exactly like the ordinary chalk and flint, that they might easily deceive a casual observer, and actually differing in scarcely anything but their fossils, and consequently their age.

But in this district there is one great quarry in which a higher stone is worked; this lies above the chalky rocks just mentioned, and thus is separated by them from the lower freestone. This higher stone is a very beautiful one; it has a brilliant white colour, and rings like a bell beneath a blow of the hammer; it is very free, very soft when first extracted, and large blocks may be obtained; moreover, to judge from the colour of the cast-out refuse, it does not easily discolour. It would seem, then, to have everything to recommend it; it is, however, somewhat coarse in the grain, and holes occur in it here and there; and, as far as can be seen, it must be very limited in quantity. Its high position in the series, and the succession of the overlying Purbecks, point it out to be on the same horizon as the true Portland Stone, so that the circumstances which favoured the production of a valuable quality may have here been repeated. The celebrated 'dirt bed,' with trees, of the Isle of Purbeck, is here repeated; the vegetable soil, the stems of the trees, but not as yet the roots.

The only remaining area in which Portland Stone is worked is the typical district which extends from Portland as far as Swanage on the east and Upway on the north. The upper surface of the Island of Portland may almost be described as one great quarry, so riddled is it everywhere by excava-

tions. The lower part of the 'stone,' resting at a considerable elevation on the 'sand,' is very flinty and of no use for building, though it represents, with the single exception above noted, the building stones of all the other districts. It is a remarkable circumstance that the flint is only developed at those places which have limestones of the same series overlying the beds, and the more of this overlying limestone there is the more flint is found. It is as if the flint were derived from this limestone and passed from it into the beds below. The upper part, however, of this flinty series contains beds which yield a very good stone, though not the best. Here the flints are few and far between, and only spoil the six or eight inches in which they lie. These beds are very inconstant, and, indeed, are more or less false-bedded; they increase in a few hundred yards to a workable thickness, and in a few hundred more become almost worthless.

The most valuable stone is known as the Whit or White bed; towards the east of the island this is seen to become false-bedded and to thin out, lying on a worn surface of the older series, but towards the west it attains a thickness of eight or ten feet, and is at its best. The grains of which this limestone is ordinarily composed are very small, and probably represent minute fragments of shells, though towards the top it is partially oolitic. Its qualities are too well known to need description, one of its chief peculiarities being its freedom from fossils; blocks of it give a fine bell-like tone, not, however, superior to that of the Tisbury stone. Over this in the island alone is found a remarkable rock, known locally as the 'roach.' It is wholly composed in one sense of shells, and yet in another sense there is not a shell in it. Originally it was a mass of shells, chiefly a doubtfully marine species, *Cerithium portlandicum*, or the 'Portland Screw.' These lay as close to one another as they could, the intervening spaces only being filled with some calcareous *débris*, subjected subsequently to some process similar to that which has dissolved the silex out of so many fossil sponge spicules and deposited it around them; the shells have all been dissolved, and only that part of the rock remains which was not shell, that is, the surrounding matrix and the internal cavity, so the roach is full of the holes where the shells have been, and contains models of their outside and inside. In this central spot—the last to emerge from out of the fostering sea—the enfeebled inhabitants of the period were congregated, and here they were together overwhelmed and found a common grave.

Although at so short a distance from Portland, neither Upway nor the Isle of Purbeck yield such good stone as the island. At the former place there is very little even to repre-

sent the building stone; almost all is chalky, and at the base flint-bearing. All along the coast from St. Alban's Head to Swanage one part or other of the series is found; but though the representatives of the upper stone may be recognized by a geologist, their value is obviously comparatively small. At the coast south of Worth-Maltravers, and at Tillym, stone is extracted, but it is obtained from a distinct bed, developed at a lower level than the White bed, and scarcely represented in the island. It is a very good freestone, white, and of fine grain, but large blocks have to be mined for.

These are the whole of the English localities where 'Portland Stone' is quarried, but the same rocks yield an abundant supply in the neighbourhood of Boulogne. Some of the stone here worked belongs undoubtedly to the same horizon as the lower beds at Portland, but the great quarries of Mont Lambert and Chatillon are said by the French geologists to belong to an earlier episode. Dr. Fitton was of a different opinion, and a hasty examination of them leads me rather to coincide with him in this. The similarity of the rocks to those at Swindon is remarkable in the extreme.

Such are the Portland building stones. One very remarkable feature about them is that they are not as a rule oolitic. Beds of very clean oolite do occur among them, consisting of the little rounded grains alone, with scarcely a particle of visible cement, but those that are of value have seldom any of these grains; they consist rather of exceedingly fine fragments, when any structure can be made out, derived from shells and other organisms. This peculiarity has been noticed by Mr. Sorby in his anniversary address to the Geological Society for 1879, and a remarkable addition made by a study of the microscopic structure. He states that the most abundant fragments are of Echinoderms, Brachiopoda, Ostreæ, Polyzoa, and Aviculæ. Now except the oyster itself, rarely associated with the actual building stone, all the others are exactly the classes of organisms which can seldom or never be found in the rocks. Echinoderms are of excessive rarity, while no one has ever seen to my knowledge a single Brachiopod, Polyzoan, or Avicula in the Portland limestone. The absence of Brachiopods especially is remarkable, though certainly they were not abundant in the Corallian limestones, yet in most limestones they are numerous. Did they exist and become universally ground to powder? or are the fragments of shells not those of animals living at the time, but derived from older rocks? If the Kimmeridge Clay were derived from the Lias, the Portland Stone might well be derived from the Bath Oolites. There is a singular absence, too, of corals; the accompanying absence of oolite lends strong support to the view that these two are intimately connected. These

observations on its structure lead to the conclusion that a large proportion at least of Portland Stone is derived not directly from broken-down shells forming calcareous mud, and subsequently consolidated, but from previously formed shelly limestones, either nearly contemporaneous or more remote in time. At all events, the name 'Portland Oolite' is to a great extent a misnomer, and for some of the rocks which are worked as building stones, 'Portland Limestone' is not much better. The great value of the stones is derived from their being so finely and uniformly ground, while their calcareous cement gives them the quality of many limestones of being easily worked, and these advantages are shared, though in a minor degree, by the beds at Worth, the Vale of Wardour, and Swindon.

CLIMBING PLANTS.*

By FRANCIS DARWIN, F.L.S.

I THINK most people have a general idea of what a climbing plant is. Even in the smoky air of London two representatives of the class flourish. A certain house in Portman Square shows how well the Virginian creeper will grow; and the ivy may be seen making a window-screen for some London dining-rooms.

Many other climbing plants will suggest themselves: the vine, the honeysuckle, the hop, the bryony, as forming more or less striking elements in the vegetation.

If we inquire what qualities are common to these otherwise different plants, we find that they all have weak and straggling stems, and that instead of being forced, like many weakly-built plants, to trail on the ground, they are all enabled to raise themselves high above it, by attaching themselves in some way to neighbouring objects. This may be effected in different ways; by clinging to a flat surface, like the ivy, or twining round a stick, like the hop, or making use of tendrils, like the vine.

These various contrivances have been studied by more than one German naturalist, as well as by my father, in whose book on the *Habits of Climbing Plants* very full details upon this subject will be found.

Climbing plants are, first of all, divided roughly into those which twine and those which do not twine; twiners are represented by the hop and the honeysuckle, and all those plants which climb up a stick by winding spirally round it. Those which are not twiners—that is, which do not wind spirally round a stick—are such as support themselves by seizing hold of any neighbouring object with various kinds of grasping organs; these may be simple hooks, or adhering roots, or they may be

* Founded on a Lecture delivered before the Sunday Lecture Society, Jan. 25th.

elaborate and sensitive tendrils, which seize hold of a stick with a rapidity more like the action of an animal than of a plant. We shall come back to this second-class of climbing plants, and shall then consider their various kinds of seizing organs. I merely wish now to insist on the importance of distinguishing between these two methods of climbing, in one of which the plant ascends a support by travelling spirally round it, in the other fixes on to the support by seizing it at one place, and continuing to seize it higher and higher up as its stem increases in length.

I have heard the curator of a foreign botanic garden bitterly complain of his gardeners that they never could learn the difference between these two classes of climbing plants, and that they would only give a few bare sticks to some tendril-bearing plant, expecting it to twine up them like a hop, while the plant really wanted a twiggy branch, up which it might creep, seizing a twig with each of its delicate tendrils, as it climbed higher and higher. These two kinds of climbers—twiners and non-twiners—may be seen growing up their appropriate supports in any kitchen-garden where the scarlet-runners twine spirally up tall sticks, while the peas clamber up the bushy branches stuck in rows in the ground.

A hop plant will supply a good example of the mode of growth of true twining plants. Let us imagine that we have a young hop plant growing in a pot; we will suppose that it has no stick to twine up, and that its pot stands in some open place where there are no other plants to interfere with it. A long thin shoot will grow out, and not being strong enough to support itself in the upright position, will bend over to one side. So far we have not discovered anything remarkable about our hop; it has sent out a straggling shoot, which has behaved, as might be expected, by falling over to one side. But now if we watch the hop plant closely, a very remarkable thing will be seen to take place. Supposing that we have noticed the shoot, when it began to bend over, pointed towards the window—say a north window—and that when we next look at it after some hours, it points into the room, that is to say, south, and again, north after another interval, we shall have discovered the curious fact that the hop-plant has a certain power of movement by which its shoot may sometimes point in one direction, sometimes in another. But this is only half the phenomenon, and if we examine closely, we shall find that the movement is *constant* and *regular*, the stem first pointing north, then east, then west, then south, in regular succession, so that its tip is constantly travelling round and round like the hand of a watch, making on an average, in warm August weather, one revolution in two hours. Here, then, is a most curious power possessed by the shoots of twining plants, which

is worth inquiring further into, both as regards the way in which the movement is produced, and as to how it can be of any service to the plant. Questions are often asked in gardening periodicals as to how hops or other climbing plants always manage to grow precisely in the direction in which they will find a support. This fact has surprised many observers, who have supposed that climbing plants have some occult sense by which they discover the whereabouts of the stick, up which they subsequently climb. But there is in reality no kind of mystery in the matter: the growing shoot simply goes swinging round till it meets with a stick, and then it climbs up it. Now a revolving shoot may be more than two feet long, so that it might be detained in its swinging-round movements by a stick fixed into the ground at a distance of nearly two feet. There would then be a straight bit of stem leading from the roots of the plant, in a straight line to the stick up which it twines, so that an observer who knew nothing of the swinging-round movement might be pardoned for supposing that the plant had in some way perceived the stick and grown straight at it. This same power of swinging round slowly comes into play in the very act of climbing up a stick.

Suppose I take a rope and swing it round my head: that may be taken to represent the revolving of the young hop-shoot. If, now, I allow it to strike against a rod, the end of the rope which projects beyond the rod curls freely round it in a spiral. And this may be taken as a rough representation of what a climbing plant does when it meets a stick placed in its way. That is to say, the part of the shoot which projects beyond the stick continues to curl inwards till it comes against the stick; and as growth goes on, the piece of stem which is projecting is, of course, all the while getting longer and longer; and as it is continually trying to keep up the swinging-round movement, it manages to curl round the stick. But there is a difference between the rope and the plant in this; that the rope curls round the stick at the same level as that at which it is swung, so that if it moves round in a horizontal plane at a uniform height above ground, it will curl round the stick at that level, and thus will not climb *up* the stick it strikes against. But the climbing plant, although it may swing round when searching for a stick, at a fairly uniform level, yet when it curls round a stick, does not retain a uniform distance from the ground, but by winding round like a corkscrew it gets higher and higher at each turn.

One may find a further illustration of the action of twining in the swinging-rope model. It is a peculiarity of twining plants that they can only ascend moderately thin supports. A scarlet-runner can climb up a bit of string, or a thin stick, an inch or

two in diameter, but when it comes to anything thicker than this, it fails to do so. Just as when the swinging-rope strikes against a large trunk of a tree, it would be unable to take a turn round it, and would fall to the ground instead of gripping it with a single turn, as it does a thin stick. The difficulty which a climbing plant has in ascending a thick stick will be better understood by going back to the original swinging round movement which the plant makes in search of a stick, and considering how the movement is produced.

As plants have no muscles, all their movements are produced by unequal growth; that is, by one-half of an organ growing in length quicker than the opposite half. Now the difference between the growth of a twining plant which bends over to one side, and an ordinary plant which grows straight up in the air, lies in this, that in the upright shoot the growth is nearly equal on all sides at once, whereas the twining plant is always growing much quicker on one side than the other.

It may be shown by means of a simple model, how unequal growth can be converted into revolving movement. The stem of a young hop is represented by a flexible rod, of which the lower end is fixed, the upper one being free to move. At first the rod is supposed to be growing vertically upward, but when it begins to twine, one side begins to grow quicker than any of the others; suppose the right side to do so, the result will be that the rod will bend over towards the left side. Now let the region of quickest growth change, and let the left side begin to grow quicker than all the others, then the rod will be forced to bend back over to the other side. Thus, by an alteration of growth, the rod will bend backwards and forwards from right to left. But now imagine that the growth of the rod on the sides nearest to and furthest from us enter into the combination, and that after the right side has been growing quickest for a time, the far side takes it up, then the rod will not bend straight back towards the right, as it did before, but will bend to the near side. Now the old movement caused by the left side growing quickest, will come in again, to be followed by the near side growing quickest. Thus by a regular succession of growth on all the sides, one after another, the swinging-round movement is produced, and by a continuation of this action, as I have explained, the twining movement is produced.

I have spoken as if the question of how plants twine were a completely solved problem, and in a certain sense it is so. I think that the explanation which I have given will remain as the fundamental statement of the case. But there is still much to be made out. We do not in the least know why every single hop-plant in a field twines like a left-handed screw,

while every single plant in a row of beans twines the other way; nor why in some rare instances a species is divided, like the human race, into right and left-handed individuals, some twining like a left-handed, others like a right-handed screw. Or, again, why some very few plants will twine half-way up a stick in one direction, and then reverse the spiral and wind the other way. Nor though we know that in all these plants the twining is caused by the change in the region of quickest growth, have we any idea what causes this change of growth. There is still much to work at, and it is to be hoped that there are still plenty of workers to solve the problems. It is by looking to exceptions that the key to a problem is often found. It is the exceptions to general rules that often lead us to understand the meaning and origin of the rules themselves; and it is to such exceptions that any one who wants to work at climbing plants should turn. Now, it is a general rule that a climbing plant twines in the same way that it revolves. It seems an obvious thing that in the case of the rope model, if we swing the rope round our head in the direction of the hands of a watch, it must twine round the stick against which it strikes in the same direction. But in plants it is not always so. In the large majority of cases it is so, for if this were not the case, the illustration of the rope would not have been applicable; but it is not universally the rule. Every individual of the plant Hibbertia always twines round its stick in the same direction, but when it is performing the swinging movement in search of a support, it is found that some plants travel round with the sun, others in the opposite direction. This fact forms an exception of a striking kind—and such exceptions are worthy of close study.

There are other facts of a different nature, which seem to show how difficult the problem is, and how delicately balanced is that part of the organization of the plant which is connected with the power of climbing. For instance, if we cut a branch of most shrubs, and put it in water, it goes on growing, apparently as healthily as ever. Indeed the practice of making cuttings—where a cut-off branch or shoot developes roots and turns into a new plant—shows us that no serious injury is thus caused. But the twining organization is sensitive to such treatment. A cut branch of hop placed in water was observed to make its revolutions in about twenty hours, whereas in its natural condition—growing on the plant—it makes a complete turn in two or three hours. Again, if a plant growing in a pot is moved from one green-house to another, the slight shaking thus caused is sufficient to stop the revolving movement for a time,—another proof of the delicacy of the internal machinery of the plant.

Some of the problems, as, for instance, why twining plants cannot as a rule climb thick stems, may be looked at from the natural-history point of view. Most of our climbing plants die down in the winter, so that if they were able to climb round big tree trunks, they would waste all the precious summer weather in climbing a few feet, whereas the same amount of longitudinal growth devoted to twining up a thin stick would have raised them up to the light after which they are striving. And as a plant exercises no choice, but merely swings round till it hits against an object, up which it will then try to twine, it seems as if the inability to climb thick stems might be a positive advantage to a plant, by forcing it to twine up such objects as would best repay the trouble.

In the classification of climbing plants, proposed by my father in his book, he makes a subdivision of 'hook-climbers.' These may be taken as the simplest representatives of that class of climbers which are not twining plants. The common bramble climbs or scrambles up through thick underwood, being assisted by the re-curved spines which allow the rapidly-growing shoot to creep upwards as it lengthens, but prevent it from slipping backwards again; the common goose-grass, (Galium) also climbs in this way, sticking like a burr to the side of a hedge-row up which it climbs. Most country boys will remember having taken advantage of this burr-like quality of Galium in making sham birds' nests, the prickly stems adhering together in the desired form. Such plants as the bramble or Galium exhibit none* of the swinging round movement which I have described in twiners: they simply grow straight on, trusting to their hooks to retain the position gained.

In some species of Clematis we find a mechanism, which reminds one of a simple hook climber, but is in reality a much better arrangement. The young leaves projecting outwards and slightly backwards from the stem, may remind us of the hooked spines of a bramble, and like them easily catch on neighbouring objects, and support the trailing stem. Or the leaf of the species of Clematis given in Fig. 1, may serve as an example of a leaf acting like a hook. The main stalk of the leaf is seen to be bent angularly downwards at the points where each successive pair of leaflets is attached, and the leaflet at the end of the leaf is bent down at right angles, and thus forms a grappling apparatus. The Clematis does not, like the bramble, trust to mere growth, to thrust itself among tangled bushes, but possesses the same powers of revolving in search of

* That is to say, the revolving movement is not sufficiently developed to be of practical importance. The same remark is applicable to the other cases in which I have spoken of the absence of revolving movement in the growing parts of plants.

a support which, simple or true twining plants possess. Indeed, many species of Clematis are actually twining plants, and can wind spirally up a stick placed in their way. And the same revolving movement which enables them thus to wind spirally, also helps them to search for some holding place for their hook- or grapple-like leaves, and in many species the search is carried on by the leaves swinging round, quite independently of the revolving movement of the stem on which they are borne.

If a leaf of a Clematis succeed by any means in hooking on to a neighbouring object, the special characteristic of leaf-climbing plants comes into play. The stalk of the leaf curls strongly over towards the object touching it, and clasps it firmly. It is obvious how great is the advantage thus gained over a mere hook. A leaf such as that shown in Fig. 2 might

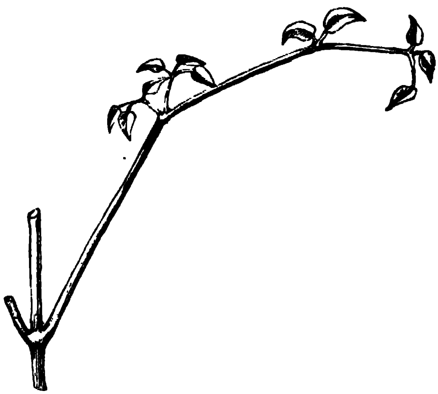


FIG. 1.*
A young leaf of *Clematis viticella*.

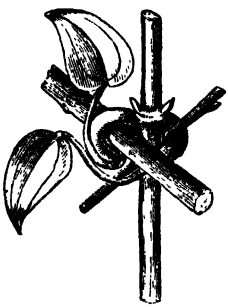


FIG. 2. *Clematis glandulosa.*
With two young leaves clasping two twigs, with the clasping portions thickened.

be made to catch on to a neighbouring twig by its bent stalk, in such a way, that although it managed to stay where it was, it could bear none of the weight of the plant, and would be liable to be displaced by a strong wind or other disturbance. But when the stalk of the leaf had curled close round the twig, nothing could displace it, and it could take its share in the work of supporting the plant.

The extreme sensitiveness of the leaf-stalk to slight and gentle touches, gives a curious idea of the alertness of the plant in its search for supporting objects. A leaf may be excited to bend, by a loop of string weighing only $\frac{1}{16}$ grain. It is an

* For the loan of this and the other woodcuts illustrating this article, we are indebted to the kindness of Mr. Charles Darwin and Mr. Murray.

interesting fact that, in such a hook-like leaf as that of *Clematis viticella* (Fig. 1), the hooked end of the leaf, which has the best chance of coming into contact with obstacles, is the most sensitive part. This has been made out by hanging small weights on different parts of the leaf, and it is found that the terminal leaflet bends in a few hours after a loop of string weighing less than a grain is hung on it, and which produced no effect in twenty-four hours on the other petioles. One may see proof of the sensitiveness of the leaf-stalks of the wild English Clematis, which sometimes catches withered leaves or delicate stalks of the quaking grass. The same thing is shown by a leaf after having been touched with a little water-colour, the delicate crust of dry paint being mistaken for something touching the plant. In such cases, or when the leaf has been merely rubbed with a twig, which is taken away before the leaf seizes it, the plant discovers that it has been deceived, and after bending for a time, it unbends and becomes straight again.

The bending, which enables a leaf to seize a twig, is not the only change which the stimulus of a touch produces. The leaf-stalk swells and becomes thicker and more woody, and turns into a strong, permanent support to the plant. The thickening of the leaf-stalks is to be made out in Fig. 2, which represents a shoot of clematis, bearing two leaves, each of which has seized a twig; in one of the leaf-stalks this thickening has commenced, and is fairly evident. The thickened and woody leaf-stalks remain in winter after the leafy part has dropped off, and in this condition they are strikingly like real tendrils.

The genus Tropæolum, whose cultivated species are often called Nasturtiums, also consists of leaf-climbing plants, which climb like Clematis by grasping neighbouring objects with their leaf-stalks.

In some species of Tropæolum we find climbing organs developed, which cannot logically be distinguished from tendrils; they consist of little filaments, not green like a leaf, but coloured like the stem. Their tips are a little flattened and furrowed but never develope into leaves; and these filaments are sensitive to a touch, and bend towards a touching object, which they clasp securely. Filaments of this kind are borne by the young plant, but it subsequently produces filaments with slightly enlarged ends, then with rudimentary or dwarfed leaves, and finally with full-sized leaves; when these are developed they clasp with their leaf-stalks, and then the first-formed filaments wither and die off; thus the plant, which in its youth was a tendril-climber, gradually developes into a true leaf-climber. During the transition, every gradation between a leaf and a [illegible] may be seen on the same plant.

It is not always the stalk of a leaf which is developed into the clasping organ; the Bignonia leaf shown in Fig. 3 bears tendrils at its free extremity. And in other plants tendrils are formed from flower-stalks, in which the flowers are not developed, or the whole stem of the plant or a single branch may turn into a tendril. In one curious case of monstrosity, what should have been a prickle on a sort of cucumber, grew out into a long, curled tendril.

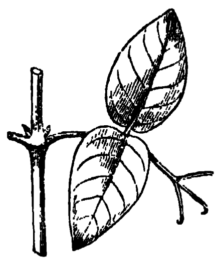


FIG. 3. *Bignonia.* An unnamed species from Kew.

The family of the Bignonias is one of the most interesting of the class of tendril-climbers, on account of the variety of adaptation which is found among them.

In the above-mentioned Fig. 3 is seen the tendril-bearing leaf of a species of Bignonia. The leaf bears a pair of leaflets, and ends in a tendril having three branches. The main tendril may be compared to a bird's leg with three toes, each bearing a small claw. And this comparison seems apt enough, for when the tendril comes against a twig, the three toes curl round it like those of a perching bird.

Besides the toes or tendrils, the leaf-stalk is sensitive, and acts like that of a regular leaf-climber, wrapping itself round a neighbouring object.

In some cases the young leaves have no tendrils at their tips, but clasp with their stalks, and this is a case exactly the reverse of Tropæolum—a tendril-climber whose young leaves have no tendrils, instead of a leaf-climber whose young climbing organs are not leaves. Thus the close relationship that exists between leaf and tendril-climbers is again illustrated.

This plant also combines the qualities of another class of climbers, namely twiners, for it can wind spirally round a support as well as a hop or any other true twiner. Another species, *B. Tweedyana*, also helps to support itself by putting out roots from its stems, which adhere to the stick up which the plant is climbing. So that here are four different methods of climbing, twining, leaf, tendril, and root climbing, which are usually characteristic of different classes of climbing plants, combined in a single species.

Among the Bignonias are found tendrils with various curious kinds of sensitiveness. The tendrils of one species exhibit, in the highest perfection, the power of growing away from light towards darkness, just the opposite to the habit of most plants. A plant, growing in a pot, was placed so that the light came in on one

side. One tendril was pointing away from the light to begin with, and this did not move; but the opposite tendril, which was pointing towards the light, bent right over, and became parallel to the first tendril. The pot was then turned round, so that both pointed towards the light, and they both moved over to the other side, and pointed away from the light. In another case, in which a plant, with six tendrils, was placed in a box, open at one side, all six tendrils pointed like so many weathercocks in the wind—all truly towards the darkest corner of the box. These tendrils also showed a curious power of choice. When it was found that they preferred darkness to light, it was tried whether they would seize a blackened glass-tube, or a blackened zinc plate. The tendrils curled round both these objects, but soon recoiled, and unwound with, what my father says, he can only describe as disgust. A post with very rugged bark was then put near them; twice they touched it for an hour or two, and twice they withdrew; but at last one of the hooked tendrils caught hold of a little projecting point of bark; and now it had found what it wanted. The other branches of the tendril quickly followed it, spreading out, adapting themselves to all the inequalities of the surface, and creeping into all the little crevices and holes in the bark. Finally a remarkable change took place in the tendrils: the tips which had crept into the cracks, swelled up into little knobs, and ultimately secreted a sticky cement, by which they were firmly glued into their places. This plan of forming adhesive discs on its tendrils is one which we shall find used by the Virginian Creeper, as its only method of support, and it forms the fifth means of climbing to be met with among the Bignonias. We see now the meaning of the power possessed by the tendrils of moving towards the dark, for in this way they are enabled to find out and reach the trunks of trees to which they then become attached. It seems, moreover, that the tendrils are especially adapted to the moss or lichen-covered trees, for the tendrils are much excited by wool, flax, or moss, the fibres of which they can seize in little bundles. The swelling process is so delicate, that when two or three fine fibres rest on the end of a tendril, the swelling occurs in crests, thinner than a hair, which insert themselves between, and finally envelope the fibres. This goes on so that the ball at the end of a tendril may have as many as fifty or sixty fibres imbedded in it, crossing each other in different directions.

The tendrils of the Virginian Creeper may here be worth noticing. This plant can climb up a flat wall, and is not adapted to seize sticks or twigs; its tendrils do occasionally curl round a stick, but they often let go again. They, like the Bignonia tendrils, are sensitive to the light, and grow away

from it, and thus easily find out where the wall lies, up which they have to climb. A tendril which has come against the wall is often seen to rise and come down afresh, as if not satisfied with its first position. In a few days after a tendril has touched a wall the tip swells up, becomes red, and forms one of the little feet or sticky cushions by which the tendrils adhere, and which are shown in Fig. 5. The adherence is caused by a resinous cement secreted by the cushions, and which forms a strong bond of union between the wall and the tendril. After the tendril has become attached it becomes woody, and is in this state remarkably durable, and may remain firmly attached and quite strong, for as many as fifteen years.

Besides this sense of touch, by which a Bignonia tendril distinguishes between the objects which it touches, there are other instances of much more perfect and incomprehensible sensibility. Thus some tendrils, which are so sensitive that they curl up when a weight of $\frac{1}{50}$th or even $\frac{1}{50}$th grain is placed on them, do not take the least notice of a shower of rain whose falling drops must cause a much greater shock to the tendrils.

Again, some tendrils seem to have the power of distinguishing between objects which they wish to seize, and their brother tendrils which they do not wish to catch. A tendril may be drawn repeatedly over another without causing the latter to contract.

The tendrils of another excellent climber, *Cobæa scandens,* possess some curious properties. The tendrils are much divided, and end in delicate branchlets, as thin as bristles, and very flexible, each bearing a minute double hook at its tip. These are formed of a hard, woody substance, and are as sharp as needles; a single tendril may bear between ninety and a hundred of these beautiful little grappling-hooks. The flexibility of the tendrils is of service in allowing them to be blown about by a breath of wind, and they can thus be made to seize hold of objects which are out of reach of the ordinary revolving movements. Many tendrils can only seize a stick by curling round it, and this even in the most sensitive tendril must take a minute or two; but with Cobæa, the sharp hooks catch hold of little irregularities on the bark the moment the tendril comes into contact with it, and afterwards the tendril can curl round and make the attachment permanent. The importance of this power of temporary attachment is shown by placing a glass rod near a Cobæa plant. Under these conditions the tendrils always fail to get hold of the glass, on which its grapple-like hooks cannot seize.

The movement of the little hook-bearing branches is very remarkable in this species. If a tendril catches an object with

one or two hooks, it is not contented, but tries to attach the rest of them in the same way. Now many of the branches will chance be so placed that their hooks do not naturally catch, either because they come laterally, or with their blunt backs against the wood, but after a short time, by a process of twisting and adjusting, each little hook becomes turned, so that its sharp point can get a hold on the wood.

The sharp hook on the tendrils of Cobæa is only a very perfect form of the bluntly curved tip which many tendrils possess; and which serves the same purpose of temporarily holding the object caught until the tendril can curl over and make it secure. There is a curious proof of the usefulness of even this blunt hook in the fact that the tendril is only sensitive to a touch on the inside of the hook. The tendril, when it comes against a twig, always slips up it till the hook catches on it, so that it would be of no use to be sensitive on the convex side. Some tendrils, on the other hand, have no

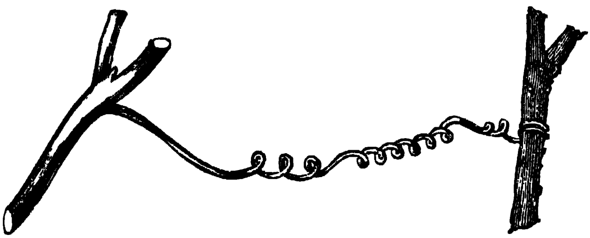


FIG. 4.

A caught tendril of *Bryonia dioica*, spirally contracted in reversed directions.

hook at the end, and here the tendrils are sensitive to a touch on any side. These tendrils led my father at first into a curious mistake, which he mentions in his book. He pinched a tendril gently in his fingers, and finding that it did not move, concluded it was not sensitive. But the fact was that the tendril being touched on two sides at once, did not know which stimulus to obey, and therefore remained motionless. It was in reality extremely sensitive to a touch on any one of its sides.

There is a remarkable movement which occurs in tendrils after they have caught an object, and which renders a tendril a better climbing organ than any sensitive leaf. This movement is called the 'spiral contraction,' and is shown in Fig. 4, which represents the spirally contracted tendril of the wild Bryony; it may also be seen in Fig. 5, which represents the tendril of the Virginian Creeper. When a tendril first seizes an object it is quite straight, with the exception of the extreme tip, which is firmly curled round the object seized. But in a

day or two the tendril begins to contract, and ultimately assumes the cork-screw-like form represented in the figures. It is clear that in spirally contracting the tendril has become considerably shorter; and since the end of the tendril is fixed to a branch, it is obvious that the stem of the bryony must be dragged nearer to the object which its tendril has caught.

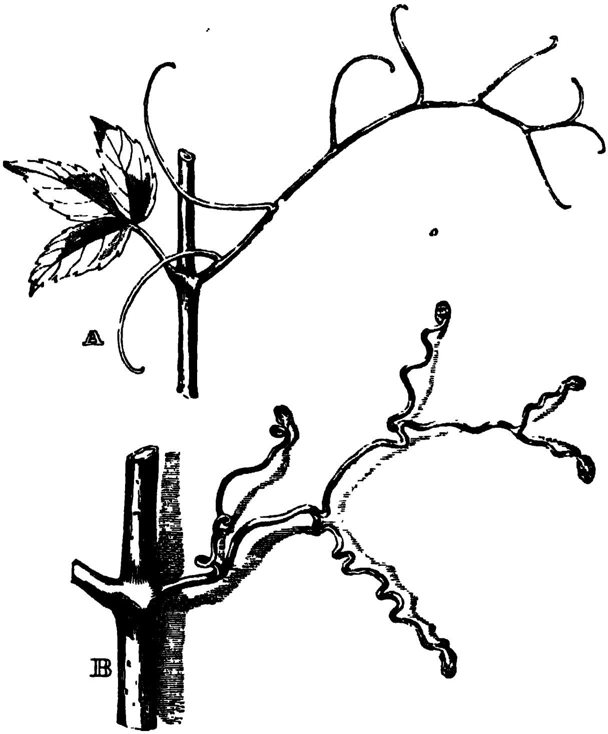


FIG. 5. *Ampelopsis hederaria.*

A. Tendril fully developed, with a young leaf on the opposite side of the stem.

B. Older tendril, several weeks after its attachment to a wall, with the branches thickened and spirally contracted, and with the extremities developed into discs. The unattached branches of this tendril have withered and dropped off.

Thus, if a shoot of bryony seizes a support above it, the contraction of the tendril will pull up the shoot in the right direction. So that in this respect the power of spiral contraction gives a tendril-climber an advantage over leaf-climbers which have no contracting power, and therefore no means of hauling themselves up to supporting objects.

But the spiral contraction of tendrils has another use,

and this is probably the most important one. This use depends on the fact that a contracted tendril acts like a spiral spring, and is thus converted into a yielding, instead of an unyielding, body. The spirally-wound tendril yields like an elastic thread to a pull which would break the tendril in its original condition. The meaning of this arrangement is to enable the plant to weather a gale which would tear it from its support by snapping the tendrils, if they were not converted into spiral springs.

My father describes how he went in a gale of wind to watch the bryony on an exposed hedge, and how, in spite of the violent wind which tossed the branches of the plant about, the bryony safely rode out the gale, 'like a ship with two anchors down, and with a long range of cable a-head, to serve as a spring as she surges to the storm.' It may also serve to divide the weight which has to be supported equally among a number of tendrils; and this is the meaning of the spiral contraction seen in the tendrils of the Virginia Creeper.

It can be seen in Fig. 4 that all the coils of the spiral are not in the same direction. First, there are two in one direction, then six in the other, and then three again in the first direction, making six turns in one way and five in the other. And this is universally the case; the turns in one direction are always approximately equal in number to those in the opposite direction. It can be shown to be a mechanical necessity that a tendril which has its two ends fixed, and which then coils into a spiral, should behave in this way.

A simple model made to show this mechanical necessity is described by Sachs in his Text Book. It is made by stretching a strip of indiarubber and cementing it to an unstretched strip. The strips being united in a state of longitudinal strain, form a spiral when released. If the model is held by one end only, the turns of the spiral are all in one direction. And this represents the behaviour of a tendril which has not managed to seize a support; for some unknown reason such tendrils contract into spirals, and the turns of such spirals are all in one direction. But if the india-rubber is held at both ends, half the turns are in one direction, half in the other, just as with a tendril the same thing happens.

Now let us consider the general relations that exist between twining plants, leaf-climbing plants, and tendril-climbing plants. To an evolutionist the question how these various classes of climbing plants have been developed is perhaps of most interest. What is the relationship between them? Have all classes been developed separately from ordinary non-climbing plants, or has one class been developed out of one of the others; and if so, which is the oldest form of climbing

plant? There can be little doubt on this latter point. I think we may certainly say that the earliest form which existed was a twining plant. We see that twining plants do not possess the essential feature of leaf or tendril-bearers, namely, the sensitiveness to a touch, which enables a leaf or tendril to grasp a stick. But, on the other hand, most leaf and tendril climbers do possess the essential quality of a twiner—the power of revolving or swinging round, which exists in the shoots, leaves, or tendrils of so many of them. This power of revolving merely serves in some leaf and tendril climbers to carry on the search for supports; but other leaf and tendril climbers, as we have seen, do actually wind spirally round a stick exactly like a true twiner. How twiners originally obtained their power of swinging round we need not now inquire; it seems to be merely an increase of a similar movement which is found to occur in a meaningless manner in other plants. Thus several flower-stems have been observed bowing themselves over and swinging round in small circles, like climbing plants. Here the movement is merely an unintelligible concomitant of growth, for, as we see, the movement is of no advantage to the flower-stem. But the existence of this movement is of great interest to us, for it shows how a twining plant might be developed by a similar movement being found to be advantageous, and being increased by natural selection to the requisite extent.

Another question which may occur to us is this: in what way is climbing by leaves or tendrils a more perfect method than twining? Why, when a plant had become a twining plant, did it not rest satisfied? The fact that leaf and tendril-climbers have been developed out of twiners, and not *vice versâ*, is a proof that climbing by leaves or tendrils is a more advantageous habit than twining; but we do not see why it should be so. If we inquire why *any* plant has become a climber, we shall see the reason. Light is a necessity for all green plants; and a plant which can climb is enabled to escape from the shadow of other plants with a far less waste of material than a forest-tree, which only pushes its branches into the light by sheer growth. Thus the weak, straggling stem of a climbing plant gets all the advantages gained by the solid, column-like tree trunk. If we apply this test,—which is the most economical plan of climbing, twining or leaf-climbing,—we see at once that a plant which climbs by seizing wastes far less material than one which twines. Thus a kidney-bean which had climbed up a stick to a height of two feet, when unwound from its support was found to be three feet in length, whereas a pea which had climbed up two feet by its tendrils was hardly longer than the height reached. Thus the bean had wasted

considerably more material by its method of climbing by twining round a stick, instead of going straight up, supported by its tendrils, like the pea. There are several other ways, in which climbing by tendrils is a much better plan than twining. It is a safer method, as any one may convince himself by comparing the security of a tendril-bearer in a heavy wind, with the ease with which a twiner is partly blown from its support. Again, by looking at those leaf-climbing plants which still possess in addition the power of twining, it will be seen how incomparably better they grasp a stick than does a simple twiner. And again, a twiner from being best fitted to climb bare stems often has to start in the shade, whereas a leaf or tendril climber can ramble for the whole extent of its growth up the sunny side of a bush.

We can thus see plainly how it has been an advantage for twining plants to develope into leaf-climbers. We shall also find reasons why a leaf-climber should find it advantageous to become a tendril-climber.

We have seen how tendrils form a more sensitive, efficient grasping organ, than simple leaves. Tendrils possess also the valuable power of shortening themselves by spirally contracting, and thus pulling up after them the stem on which they grow; and afterwards serving as springs and breaking the force of the wind. We have had some cases where we see the close relationship between leaf and tendril-climbers, and where we can see intermediate stages in the process of transition from one method of climbing to the other.

In certain kinds of Fumaria we can follow the whole process. Thus we have one kind, which is a pure leaf-climber, grasping by its leaf-stalks, which bear leaflets not at all reduced in size. A second genus has the end leaflets very much smaller than the rest. A third kind has the leaflets reduced to microscopical dimensions; and lastly, a fourth kind has true and perfect tendrils. If we could see the ancestors of this last kind we should undoubtedly have a series of forms connecting it with an extinct leaf-climber, resembling the series which at present connects it with its contemporary leaf-climbing relatives.

To repeat once more the steps which it is believed have occurred in the evolution of climbing plants. It is probable that plants have become twiners by exaggerating a swinging-round or revolving movement, which occurred in a rudimentary form, and in a useless condition, in some of their ancestors. This movement has been utilized for twining, the stimulus which has driven the process of change in this direction having been the necessity for light.

The second stage has been the development of sensitive

leaves by a twining plant. No doubt at first no leaf-climber depended entirely on its leaves, it was merely a twiner which helped itself by its leaves. Gradually the leaves became more perfect, and then the plant could leave off the wasteful plan of growing spirally up a stick, and adopt the more economical and more effective one of pure leaf-climbing.

Finally, from sensitive leaves were developed the marvellously perfect tendrils which can perceive $\frac{1}{50}$th of a grain, and can show distinct curvature within 25″ after being touched, tendrils, with delicate sticky ends, or endowed with the power of moving towards the dark, or of creeping into little cracks, or with that mysterious sense of touch by which a tendril can distinguish a brother tendril from an ordinary twig, and can distinguish the weight of a drop of rain hanging to it from a bit of thread—in short, all the delicate contrivances which place tendril-bearers so eminently at the head of the climbing plants.

There is only one more fact connected with the evolution of climbing plants which must be alluded to, namely, the curious way in which the representatives of the class are scattered throughout the vegetable kingdom. Lindley divided flowering plants into fifty-nine classes, called Alliances, and in no less than thirty-five of these climbing plants are found. This fact shows two things: first, how strong has been the motive power—the search after light—which has driven so many distinct kind of plants to become climbers. Secondly, that the power of revolving, which is the first step in the ladder of development of the power of climbing, is present in an undeveloped state in almost every plant in the vegetable series.

ON THE INFLUENCE WHICH A MOLECULAR MOVEMENT DUE TO ELECTRICITY MAY HAVE EXERTED IN CERTAIN GEOLOGICAL PHENOMENA, NAMELY, THE METAMORPHISM OF ROCKS AND THE FORMATION OF METALLIFEROUS DEPOSITS.*

By MANUEL FERNANDEZ DE CASTRO.

GEOLOGISTS of the present day admit three distinct theories concerning the formation of metalliferous deposits: 1. One theory for metalliferous deposits, such as pockets, veins, stockwerks, and segregated lodes, the grouping or concentration of which is ascribed to molecular action, but without explaining the origin of the latter. 2. Another theory for true plutonic deposits, including also almost all lodes of contact and often showing molecular grouping. 3. A third theory for the true or concretionary lodes, the component parts of which are believed by some geologists to have been brought, dissolved by thermal waters, from the interior of the earth. Some geologists, however, consider these lodes due to segregation.

The question arises: Is there no possibility of establishing a closer relation between these three classes of deposits?

Although many of the phenomena observed in mineral deposits could be more satisfactorily explained by the hydrothermal than by the plutonic theory, the former left many difficulties. Amongst others, as pointed out by De la Beche, the difference noted in the composition or structure of the lodes according to the nature and character of the rock traversed; and further, the fact that we may find in the mass of the deposit, close to substances due to the agency of water, others which could be produced artificially only in the dry way, and at very high temperatures. The last-mentioned difficulty has been overcome to a certain extent by Senarmont, who succeeded in producing, by the agency of water, at a temperature varying

* Extract from a pamphlet, *Discursos leidos ante la Real Academia de Ciencias, etc. en la recepcion publica del Exmo. Sr. Don Manuel Fernandez de Castro*. Madrid, 1878.

from 130° to 200° C., the principal characteristic minerals of lodes, such as quartz, spathic iron ore, heavy spar, and ruby silver. By far the greatest support, however, was given to the hydrothermal theory by the remarkable discovery, in the walls of a Roman aqueduct at the thermal springs of Plombières, of a Roman bronze key, covered with crystals of copper-glance, identical with those occurring in the mines of Cornwall. The waters of Plombières have a temperature of 70° C., and contain small quantities of silicates and sulphates of potash and soda. Crystals of other minerals, such as hyalite, apophyllite, and chabasite, were discovered in cavities of the mortar, but are not found in the porphyritic granite, from which these thermal waters flow. Daubrée carefully studied the occurrence of these minerals at Plombières, and, by experiments made in consequence, he proved that by means of water under a high pressure and at a temperature of 400° C., not only can quartz be obtained in crystals, as already shown by Senarmont, but also crystals of anhydrous silicates, such as felspar, diopside, and wollastonite. Daubrée considers the formation of the greater portion of the metalliferous lodes as a particular case of metamorphism, chiefly due to the concurrent action of heat, water, and pressure. These conclusions were put forth in an essay read before the Academy of Sciences of Paris in 1859; but ten years later the same scientific body awarded a prize to Delesse, who had arrived at the conclusion, that the causes of general or local metamorphism are those met when penetrating into the interior of the globe, viz., heat, water, pressure, and, above all, molecular action.

The formation of the minerals found at Plombières has taken place, however, without a pressure above that of the atmosphere, and without the direct action of the central heat of the globe, and is undoubtedly due to the slow but continuous action of a very small quantity of water of a temperature not above 70° C. The only manner in which the formation of these various minerals can be explained is by molecular action, as first suggested by Elie de Beaumont. As regards the nature and origin of this so-called molecular action, no explanation is given by the various authors; and De Castro finds this rather remarkable, considering that one author cites in his writings Becquerel, who as early as 1823 had demonstrated the influence of slow actions produced by electricity of a very feeble tension in connection with the precipitation of insoluble combinations similar to those found in nature. This manner of considering electrical reactions between molecule and molecule, quite different from that generally current, is worthy of attention, because it may be said that all the theoretical reasoning founded upon the known effects of electricity has been justified

by experience. These effects are as manifold as the causes which produce electricity, or as the circumstances under which it is set free and propagated. Considering the results obtained by means of electro-chemical action from the time that Nicholson and Carlisle decomposed water, and Davy extracted potassium and sodium by means of the voltaic pile, up to the time when Despretz obtained real crystals of diamond with the Ruhmkorff coil; and considering further the numerous crystallized substances, identical with natural ones, artificially produced by Becquerel and Crosse; it would be unreasonable to reject the idea of establishing a theory of metamorphism based upon electrochemical and electrodynamical actions. All the effects due to metamorphism can also be produced by electro-telluric action. Thus Alcide d'Orbigny, when treating of fossilization in his well-known *Cours Elémentaire de Paléontologie*, says that electrical actions and chemical affinities furnish excellent means for explaining substitution in fossils; and he adds at the same time, that the action of these occult forces is more general than has hitherto been believed. Becquerel succeeded in reproducing by electrochemical processes epigenics of petrefactions, which cannot be produced artificially in any other way; and when enumerating the causes to which metalliferous lodes are due, Fox and Fournet speak of electrochemical actions resulting from the contact of a number of various rocks.

The author's attention always has been drawn to certain geological phenomena which can be reproduced artificially in the laboratory of the chemist, but only by employing much more powerful means than nature apparently makes use of for the same purpose. Although we are able to perceive in both cases the effects, it is different as regards the *modus operandi*, because our senses are only capable of distinguishing and appreciating phenomena within a limited scale; and as soon as we have to consider something very great or very small, very quick or very slow, we require the use of instruments which increase the faculty of perception: but in the greater number of cases we can thus realize, by our imagination only, the nevertheless well-established facts. As already mentioned in the artificial production of the minerals found at Plombières, a much higher temperature and pressure had to be used than nature apparently employed in their formation. Thus, Wöhler had to make use of a temperature of 180°–190° C., and of a pressure of from ten to twelve atmospheres to produce artificially those minerals which nature had formed in the Roman aqueducts of Plombières at a temperature of 70° C., and under ordinary atmospherical pressure. Is it possible that nature, operating by infinitesimally small actions, but during an unlimited time, can achieve results astonishing by their magnitude, just as in the higher

mathematics, by the integration of the infinitesimally small, real and absolute quantities are obtained? Tyndall, in his examination of the origin and probable destiny of the Niagara falls, when speaking of the wear and tear due to running water, states that time and intensity are the principal factors of geological change, and, to a certain extent, convertible into each other. A weak force acting during a long period, and a more intense one acting during a shorter period, may approximately produce the same effect. The sand-blast, invented by Quincey, is an example of the concentration of force in space; and the reciprocal action of steel and flint, an illustration of the same principle. Although the total amount of heat produced by striking softer substances with each other, such as calc-spar and lead, may be even greater, the generation of a spark requires a local concentration of heat. This local concentration of heat is also obtained in electro-calorific actions. By the use of a Voltaic pile of 2000 pairs, Sir H. Davy succeeded in melting platinum; and though it can be demonstrated that, in a given time, the chemical action taking place in this pile is far from being equivalent to the fuel consumed in the same time in a blast-furnace, the heat generated in the latter is insufficient to melt the platinum-wire, which melts after being exposed for some seconds between the electrodes of the Voltaic pile. The Voltaic arc acts similarly to a powerful blowpipe, which heats and melts in rapid succession the particles of platinum; whereas, in a blast-furnace, the heat generated cannot accumulate in a given point, and therefore is incapable of melting the platinum, *i.e.*, of producing, in each of its molecules, the number of vibrations which are necessary for its melting.

If the possibility of calorific effects of the electric currents is admitted in the same manner as the electrochemical effects are already universally accepted for the purpose of explaining the slow reactions which operate in the earth's interior, all the principal difficulties presented by local and contact metamorphism disappear, and it then becomes possible to explain, without having need of any violent theory, such notable cases as, for example, that of the Blaue Kuppe, near Eschwege, to which Delesse repeatedly draws attention in his *Etudes sur la Métamorphisme*. In that locality, well known to geologists, is found a basaltic conglomerate which encloses a great number of small fragments of a variegated sandstone, which are all vitrified on their outside. This effect cannot be attributed to the basalt, because the heat retained by such a comparatively small mass would not have been sufficient to melt the exterior of the sandstone fragments. Delesse, therefore, arrives at the conclusion that an agent more subtle than the basalt has penetrated the sandstone, and produced the same liquidity by fusion.

According to his idea, this agent was water; but De Castro considers it not more venturous to attribute the phenomena of the Blaue Kuppe to the calorific action of the electrochemical and electrodynamical currents, produced by that same water infiltrating into the sandstone, or by the different temperature of the contact rocks, or by any other of the many causes which incessantly originate analogous currents. For the explanation of local metamorphism, extending over large districts situated at a great distance from the fluid interior of the earth or the eruptive rocks, and sometimes containing interstratified, not altered rocks, the hydrothermal theory cannot be admitted without exaggerating the physical properties of the substances, and attributing to them properties which we are not aware of. But in the case of local metamorphism, all effects due to molecular action are observed—effects, which are all capable of producing electro-telluric currents. An electrical current, by whatever cause produced, be it by the contact of two rocks of a different temperature, or the slow circulation of water more or less charged with other substances, or the very act itself of rending or fissuring the earth's crust by the contraction or sliding of the rocks, or the presence of some organic substance in the layers of the formation, or many other causes, can circulate with more or less resistance through the whole of the terrestrial mass and will only cease when, according to the law of the indestructibility of forces, it is changed into another force which manifests itself by thermal, chemical, or mechanical effects. Becquerel explains by electricity many phenomena of cementation which are observed in nature; and if one compares his simple and reasonable theory with that proposed by Plattner as an explanation of the formation of 'kernels,' when calcining certain cupriferous minerals, it is not possible to doubt about the real cause which produces 'kernels' containing 40 per cent of copper in the calcination of the Rio Tinto ore, of which even the richest portion seldom reaches 8 per cent. The electro-telluric actions may have played an important part in a number of geological phenomena, in the formation of the nodules and bands of flint in the Chalk, differently explained by Gaudry, Hébert, and d'Orbigny; the grains of oxide of iron in the Tertiary; the oolitic iron-ore in the Jurassic deposits; the sphero-siderite of the Carboniferous formation and the geodic iron ore of the modern formation; the kidneys of phosphorite, which appear in distinct geological epochs; further, in the concentrical structure of certain limestone rocks and calcareous minerals; the tendency of rocks to assume a spherical form; the curious formation of embossed pebbles; the eaglestones, septaria, and other phenomena, during a considerable time considered merely as *lusus naturæ*; the formation of agates, the delicate layers

of which have a regularity and symmetry in their grouping which it is impossible to attribute to successive infiltrations through an orifice, merely imaginary in most cases. Electro-telluric actions play a considerable part in the formation of the concretionary lodes, which, according to De la Beche, can only be considered contemporaneous with the rocks in which they occur. The same remark applies, also, to the segregated lodes which Weissembach and English geologists explain as a grouping of the particular minerals disseminated in the containing rocks. Admitting that the metals and other substances which fill the rents and fissures in the earth's crust, and also the waters which have carried them, do not exclusively originate from the interior of the earth, but from the whole of the surrounding mass, from the surface to the lowest part of the rent or fissure; and further, that by the slow and successive entrance of these substances as well as by the circulation of the waters, and other previously mentioned causes, electrotelluric currents have been produced, all the phenomena of metamorphism, as regards the lodes, can be explained, and thus the greatest difficulties which this arduous problem presents can be made to disappear. The comparative greater richness of lodes running across the stratification than of those parallel to it, and also the well-known fact of an increased richness at the crossing of two lodes, can be explained by a relative greater display of electrotelluric action.

The reason why all the phenomena are attributed to the electrical force, which until now have been explained by molecular action, electrical repulsion, chemical affinity, and other causes, is the author's belief in the unity of the various natural forces. If physicists of great distinction have now arrived at the conclusion to attribute the light and heat emanating from the sun to continuous electrical discharges in his photosphere, as well as in his interior mass, one can well ascribe to electricity the actions which take place in the metamorphism of rocks, instead of considering it only, according to some geologists, as a secondary force, because heat, pressure, and other modes of force, produce electricity. Electricity, by itself, can not only originate these, but also other manifestations of molecular motion. In the same manner as the immense number of generations of infusorial animals have succeeded in building up monuments in rock, in comparison with which the grandest human constructions are mere playthings, the almost imperceptible electrical currents which exist in the earth's shell are able, by their number and continuity, to produce changes which neither man is able to bring forth, even when aided by the most powerful resources of science, nor nature herself could exceed, should she prefer to give us proofs of her power by changing the face of the earth by means of terrible cataclysms.

SUNSPOTS AND BRITISH WEATHER.

By W. L. DALLAS, of the Meteorological Office.

THE forecasts which appear in our daily papers refer only to a space of about twelve hours after their time of issue, and if they give us a correct idea, which as a rule they do, of the weather of the approaching day, they do all that was ever claimed for them, and their object is attained. Whatever may be the value of such information (and during harvesting operations it must be enormous), no one would probably deny that it is of much more importance to be able to give warning of a dry or wet season, than merely of a dry or wet day. When the seasons have their normal amounts of rain, cloud, and sunshine, the present system of forecasts may be all that is required, but when we are passing through such a spring and summer as was experienced last year, it is felt that something more is wanted, and the necessity of seasonal forecasts is fully recognized. The middle six months of 1879 were certainly unexampled for many years past for coldness and continuance of rain, yet we had no previous warning that such would be the case, and practical meteorologists were quite unable to say whether the showery, unsettled weather, when it had once set in, would last for only one month, or for two, or till the end of the year. In consequence, the capital of our agricultural community was as thoroughly lost as though it had been thrown into the sea. Hundreds of thousands of pounds that might have been profitably employed in some other way, or at least might have been laid by for a more favourable season, were expended in labour that was to prove quite abortive, and in seed that was destined to be washed away. It is now quite evident that while telegraphic reports of the actual state of atmospheric pressure, the winds, &c., may and do enable us to form a very correct estimate of the probable weather for several hours in advance, their indications are unsafe guides for any longer period, so that it becomes clear, that in order to succeed in forecasts

for, say, three or six months in advance, meteorologists must succeed in proving a connexion between our weather and some extra-terrestrial or other phenomenon which, being regular and periodic in its variations, would give us the necessary warning of approaching climatic change.

The apparent success attending the work of the Sunspot theorists, induced me to take a 'Sunspot period,' and by carefully working through it, having regard to the meteorological elements of rainfall, temperature, and weather, to find out whether some parallelism might not be discovered between the number of Sunspots and the weather experienced. Everyone is aware that nearly every important climatic alteration is due, directly or indirectly, to cosmical changes; and the first efforts of the advocates of the Spot theory having apparently been crowned with success, droughts, floods, and more recently commercial crises, have been ascribed to the presence or absence of spots on the surface of the Sun.

It is now matter of everyday knowledge, from the investigations of Wolf, Balfour Stewart, and Lockyer, that the number of spots which appear on the surface of the Sun has a regular period of increment and decrement, and that these changes proceed during a cycle of about 11·1 years. For long these spots were believed to be cooled solid bodies, or even clouds sailing about on the surface of the Sun, but it is now supposed that they are vortices in the surrounding atmosphere, caused by the descent of upper and cold currents into the interior of the Sun. Their numbers consequently indicate increased activity in the Sun's atmosphere, and hence, probably in solar radiation; and as all changes in our atmosphere depend on the intensity of solar radiation, the importance of their indications cannot be too highly estimated. It happens, unfortunately, that considerable divergences of opinion exist among the authorities who have investigated this matter. Some, among whom are Messrs. Baxendell and Blanford, maintain that solar radiation is much more intense in years of Sunspot maximum than in those of minimum, believing, in fact, that the spots themselves are indications of the intensity of solar radiation; while other writers, relying on the observations of Dr. Köppen, believe that while the electrical phenomena of our atmosphere are in some manner intensified according to the abundance of Sunspots, periods of high air temperature correspond rather with years of minimum than of maximum spots. A decision on this important point would assist considerably in all discussions as to the effect of these cosmical changes on the meteorology of the Earth; but until the establishment of observatories, in such almost cloudless districts as some parts of Africa, Arabia, or India, so that actinometrical observations can be continued for long periods under almost unvarying

conditions, it is difficult to believe that the subject can be satisfactorily settled. This uncertainty, however, does not much affect the present paper, the object of which is, not to find out either the origin or formation of the spots, but to ascertain whether the presence of greater or less numbers of spots on the surface of the Sun affects our atmosphere in such a manner as to cause a more or less marked periodicity in our climatic changes.

In Ceylon, the variation of the rainfall, and consequently of the cereals, in direct ratio with the spots, has become a matter of popular observation; but so far as we know, this is the only part of the Earth's surface in which the variation is so noticeable. Lying as Ceylon does, almost under the equator, it is of course there that we should expect to find any changes occasioned by alterations in the amount of solar radiation exceedingly strongly marked, and that this is the case is a great argument in favour of the upholders of the Sunspot theory. In Mr. Blanford's *Vade Mecum,* where rainfall and Sunspots are investigated with a minuteness commensurate with the importance of the subject to the Indian community, the rainfall of a long series of years is given for six important Indian stations. This series is subdivided into eleven-year periods, and the average rainfall of each of the homonymous years being taken, the difference of each average year is shown as a percentage of the local mean of each station, and in a second table as a percentage of the mean rainfall of all the six stations. The latter table, more particularly when the amounts have been smoothed down by a process which will be explained fully later, shows a very distinct variation in the amount of rain, a decided excess in years of Sunspot maximum, and a decided deficit in those of Sunspot minimum. The following figures, copied from the *Vade Mecum,* show the rainfall for Madras, and it is impossible to deny that there is very strong evidence of a connexion between the two phenomena of precipitation of rain and of the presence or absence of spots on the surface of the Sun:—

Year.	Inch.	
1860, &c.	+ 8·5	per cent.
1861 "	+ 6·2	"
1862 "	+ 9·2	"
1863 "	+ 5·8	"
1864 "	− 7·1	"
1865 "	− 13·2	"
1866 "	− 12·7	"
1867 "	− 14·0	"
1868 "	− 6·0	"
1869 "	+ 6·0	"
1870, &c.	+ 11·7	"

The group of years under 1866 being that of average Sunspot minimum, and that under 1870 being that of Sunspot maximum, the above figures show a very distinct variation in accordance with the relative number of Sunspots, a difference of 25 per cent existing between the rainfall of the series of years of maximum spots, and that of the series of minimum spots. While, however, quoting this table, which apparently shows incontestable evidence of a parallelism between rainfall and Sunspots, it must be borne in mind that the returns are for one station only, and for a station which, just as in the case of Ceylon, must, from its geographical position, be peculiarly susceptible of any changes in the direct action of the Sun. In other places, even in India, no such marked cyclical variation is shown; and it is remarkable that one of the most severe famines which has been experienced in Northern India, occurred in 1837–8, and was directly attributable to scarcity of rain, during two years which were those of maximum Sunspots.

With such incongruities in India, what was to be hoped for in the British Isles? Any cyclical variation, as well as our smaller atmospheric changes, all depend for their motive force on solar radiation. The force of the wind and its direction, the rain and the fine weather, all depend directly or indirectly on the amount of heat we receive from the Sun; and when this is remembered, it will be understood how variations, which may even be strongly marked under the equator, are hardly noticeable in the north-west of Europe. In dealing with different zones of the Earth's surface, however, it must be borne in mind that the very same causes which may occasion certain changes in the tropical regions may exert an influence in a directly opposite direction in the temperate zones, so that to look for an exact parallelism between the secular changes in the tropics and those in other parts of the Earth's surface, is to disregard entirely the enormous physical differences which exist between those respective regions, and in fact ignore the heat of the Sun, the prime mover of all the variations in our atmosphere. Turning back to the Madras register, it is remarkable that the year of minimum rainfall is not the same as that of minimum Sunspots, but occurs one year later. If then, at Madras, it is found that the cycle of rainfall is one year later than that of Sunspots, we, in the temperate zone, might be prepared for a much greater discrepancy; the experience of the tropics might even be reversed in England, and yet the theory of the advocates of the Sunspot school remain practically unshaken. Let it be supposed that the heat radiated from the Sun is much less in years of Sunspot maxima than in those of Sunspot minima, it is then fair to assume that the general wind over the surface of the Earth would be less strong, so that in consequence

a large amount of precipitation would occur in the tropics, the enormous amount of water which is daily evaporated there not being carried quickly away to other regions. Such a hypothesis would satisfactorily explain an excess of rain in the tropics, and a deficit in the temperate zone in the same year, a condition of affairs, which, without some such explanation, would be fatal to the Sunspot theory. From the works of some of the most eminent of the Sunspot theorists, however, it appeared that any such argument was quite unnecessary, the rainfall of different stations, collected from independent sources, displaying an apparently exact agreement with the number of spots. Taking Edinburgh, we find that the figures quoted by Mr. Meldrum show a remarkable parallelism between the number of spots and the amount of rain. The period under discussion extends from 1824 to 1867, and each year's rainfall has been classified according to its position in the series as a year of maximum, minimum, or intermediate Sunspots, so that the variation from the mean rainfall of all the years of maximum Sunspots is shown by the figures in the fifth year of the series, and the variation of all the years of Sunspot minima by the figures in the first or last lines:—

Rainfall. Variation from the Mean. Inches.	Sunspots. Variation from the Mean
− 2·8	− 37·2
− 1·8	− 22·8
+ 0·7	+ 4·4
+ 2·4	+ 33·0
+ 3·3	+ 43·8
+ 2·8	+ 32·9
+ 0·5	+ 14·3
− 0·4	− 2·9
− 1·0	− 16·6
− 2·5	− 24·7
− 1·7	− 24·0

From this table it was hoped that the general rainfall of the British Isles might be found to vary regularly according to the increment and decrement of the Sunspots; but it is never safe to take the rainfall of any one station, and conclude that it properly represents a large district, and gives the normal rainfall for that part of the Earth's surface. It happens, however, that the Meteorological Office has lately issued a small pamphlet, in the form of a supplement to the Weekly Weather Report, in which are given the best available averages for fourteen years of temperature and rainfall, not for particular stations, but for the different meteorological districts into which the British Islands have been divided. By accepting these tables, we, in a great

measure, do away with local peculiarities, and have a fair idea presented of the general rainfall and temperature over the kingdom. The eleven-year cycle, which we find embraced by the fourteen years quoted in the pamphlet, runs from 1867 to 1877, and the following is the relative number of Sunspots according to Wolff in this period:—

1867	8·8	per cent.
1868	36·8	"
1869	78·6	"
1870	131·8	"
1871	113·8	"
1872	99·7	"
1873	67·7	"
1874	43·1	"
1875	18·9	"
1876	11·3	"
1877	7·0	"

It will be noticed that the relative number of Sunspots rises from a very small number in 1867 to a very considerable one in 1870, and then decreases again to about the original figure by 1877. The increase is, however, much more rapid than the decrease, and it has been proved by Mr. Meldrum that the epoch of maximum number of Sunspots occurs on the average about four years after the epoch of minimum, and the epoch of minimum about seven years after the epoch of maximum number. So that though the cycle is one of eleven years, yet that period is not divided into two equal parts, during one of which a regular annual increment occurs, and during the other an equally regular decrement; but into a period of four years of increment and seven of decrement. In the cycle under discussion, the epochs of maximum and minimum number of Sunspots are fortunately more strongly marked than they have been at any time during the past fifty years, so that if the actual number of Sunspots have any decided effect on our weather, the results should be more strongly accentuated during these years than at any other time. The following figures show the mean rainfall of the British Isles, divided into eastern and western divisions, and for the Islands as a whole:-

	East.	West.	British Isles generally.
1866	34·8	46·9	40·3
1867	31·2	41·0	36·0
1868	33·2	44·4	38·3
1869	32·5	42·4	37·0
1870	25·6	35·3	30·0
1871	29·4	39·7	34·1
1872	41·8	58·0	49·2

	East.	West.	British Isles generally.
1873	29·6	39·3	34·0
1874	29·5	41·4	34·9
1875	32·9	42·3	37·2
1876	34·5	44·1	38·9
1877	37·2	52·7	44·2
1878	29·9	39·1	34·1

It will be noticed that the years 1866 and 1878 are quoted in the above list, as, by taking these two years, we are enabled to make use of a smoothing process, by means of which the rainfall is welded, as it were, into one continuous band. In drawing the curve *a*, in fig. 1, the following plan has been adopted: Let *a*, *b*, *c*, &c. equal the first, second, third, &c. annual amounts, then $\frac{a+2b+c}{4}$ gives the amount of rainfall which is taken to represent the rainfall of the first year of the curve, and so on throughout the whole series. In the second curve (*b*) the actual amount for each of the years is given under its respective date, and no attempt at smoothing down is made, but it will be noticed that the two curves show a very close resemblance to one another, the difference being only in degree:—

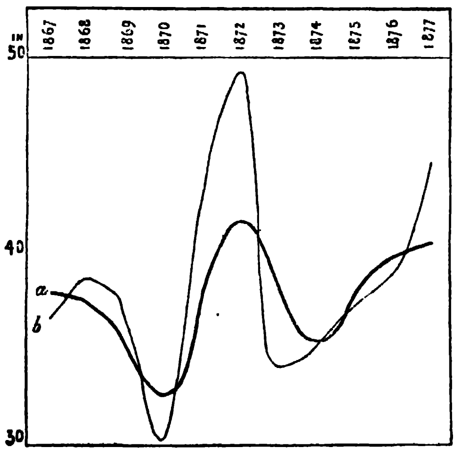


Fig. 1. Curves showing the Annual Rainfall in the years 1867–1877 inclusive.

From these curves it will be seen that so far from the year of maximum rainfall being that of maximum Sunspots, the reverse is the case, the year 1870, which has an exceptionally large relative number of Sunspots, showing the smallest amount

of rainfall. This might be explained by the hypothesis, proposed above, but supposing such to be the case, then the years 1867 and 1877 should be years of considerable rainfall. This, however, is far from being the case, the years at the extremity of our cycle showing only a very slight excess over the mean rainfall for the period. The year 1872, however, which is only two years after the year of maximum Sunspots, and should, according to the above reasoning, show a pronounced deficit, has the heaviest fall of any year during the whole period, the total amount of rainfall exceeding by nineteen inches that which was collected in 1870. It will also be noticed that both 1867 and 1877 are years of Sunspot minima. If then we take 1869 and 1879, and discuss their rainfall, we are taking two years which are distinctly comparable, their positions in the eleven-year cycle being exactly the same. It will, however, be found that fifty per cent more rain fell in the six months April to September, 1879, than fell in the same six months of the year 1869, so that a prophecy, based on the rainfall of 1869, could have given us no warning of the amount of rain with which we were to be visited in 1879. The following table gives (1) the actual amounts of rainfall measured within the British Isles during the eleven years under discussion; (2) the amounts smoothed down by the above-mentioned process; (3) the difference of each year shown as a percentage of the mean rainfall; and (4) the relative number (also smoothed down) of Sunspots for each year:—

	In.	In.	Per-centages.	Sunspots
1867	36·0	37·7	+ 0·3	8·8
1868	38·3	37·4	− 0·5	36·8
1869	37·0	35·6	− 5·3	78·6
1870	30·0	32·8	− 12·8	131·8
1871	34·1	36·9	− 1·9	113·8
1872	49·2	41·2	+ 16·4	99·7
1873	34·0	38·0	+ 1·1	67·7
1874	34·9	35·3	− 6·2	43·1
1875	37·2	37·1	− 1·3	18·9
1876	38·9	39·8	+ 5·8	11·3
1877	44·2	40·4	+ 7·4	7·0

Leaving rainfall and turning to temperature, the same process has been adopted with the mean temperatures of the eleven years as that adopted with amounts of rainfall, but the mean temperature shows a course as irregular as that of rainfall, nor do the two together show any peculiarities in common (see fig. 2).

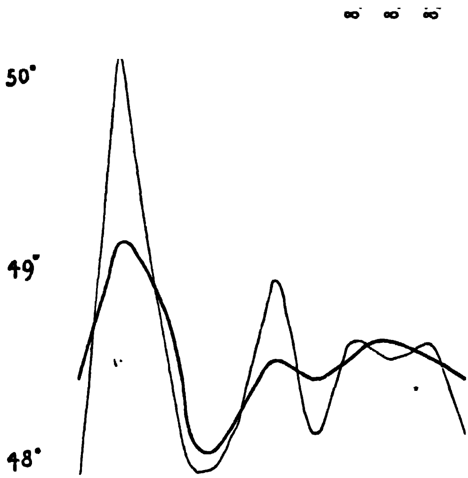


FIG. 2. Curves showing Annual temperatures in the years 1867–1877 inclusive. As in fig. 1, the darker line is the smoothed curve.

Whatever may be the result of the actinometrical observations which, it is to be hoped, may one day be made in one of the almost cloudless districts of the world, it certainly appears that there is no concord whatever between the relative number of Sunspots and the heat radiated on the British Isles. The hottest year of the whole series was 1868, its six central months being quite exceptionally warm, but if we attempt to connect that fact in any way with Sunspots, we find that it was the first year after a year of minimum Sunspots; but on passing on eleven years, and taking the first year following the next Sunspot minimum, we find that it was in no way remarkable for excessive heat. Following on to the next year, that is to say, the second after the year of Sunspot minimum, we find 1869 with a temperature very near the mean; but on again passing on eleven years, we come to 1879, which was considerably the coldest year that has been experienced for a very long time.

Taking up the question of the heat of the summer months of the different years, we are unable to derive any great comfort or hope from an examination of their mean temperatures. It would have been fair to expect that in a curve of the mean summer temperatures, some sort of evidence of the existence of a cycle might be traced, even though in a curve of the temperatures of the whole year its existence might be hidden by the

readings taken in the winter. This, however, is not apparently the case, the differences which do occur seemingly following no rule. The summer temperature of 1867 was decidedly low; but that of 1868 was decidedly high; from 1868 until 1873 the summers, with slight oscillations, became colder and colder; from 1873 to 1875 they became slightly warmer again; but, with 1876 a fall set in, and the summer of 1877 was the coldest we have experienced for many years past.

The winter means display equally inexplicable irregularities. The years 1868, 1869, 1872, 1874, and 1877, enjoying more or less mild winters, and the years 1867, 1870, 1873, and 1876, experiencing, more or less, severe weather. In the years 1867 and 1877, which are both years of Sunspot minimum, it will be noticed great differences exist in the winters; that of 1867 is the coldest of the whole period, whilst that of 1877 is among the warmer ones. The following curves (fig. 3) give the actual mean temperatures of the summers and winters of this period. The first curve with the scale on the left gives the winter, and the second curve with the scale on the right the summer means.

In conclusion, it has been attempted to connect the electrical phenomena of our atmosphere with solar physics; but with the same unfortunate result. The average number of days on which thunderstorms occurred during each summer of the period was forty-nine; and the following table shows, for each year, the departure from that mean:—

In 1867	. . . . . .	− 5 days.
1868	. . . . . .	+ 4
1869	. . . . . .	− 5
1870	. . . . . .	− 16
1871	. . . . . .	+ 12
1872	. . . . . .	+ 20
1873	. . . . . .	− 6
1874	. . . . . .	0
1875	. . . . . .	+ 5
1876	. . . . . .	− 6
1877	. . . . .	− 3 "

These figures, like the former, give no evidence of any variation, either with or against the number of Sunspots. Both 1867 and 1877, the years of Sunspot minimum, show a slight deficit in the number of days with thunder; but on the other hand, 1870, which is the year of maximum Sunspots, shows a much more considerable falling off, while 1871 and 1872 have a very large excess.

With the whole mass of evidence before us then, it becomes almost imperative to confess that, as practical guides in fore-

casting weather, Sunspots must remain of very secondary importance. It must be remembered, that science, like politics, has a practical and a purely theoretical side; and that while

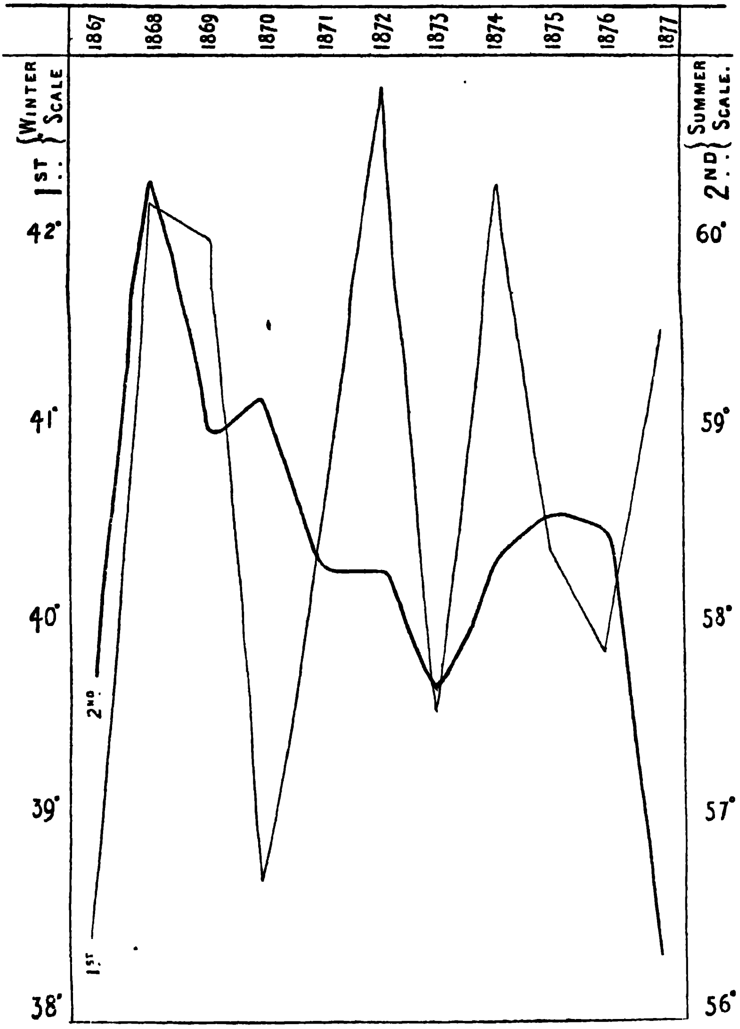


FIG. 3. Curves showing Summer and Winter temperatures in the years 1867–1877 inclusive.

evidence of the correctness of a theory, even if it be of the very slightest character, is a reward amply sufficient to the theoretical student, proof that the theory is of practical importance is

required to arouse the enthusiasm of another class of workers. From a practical point of view, Sunspots seem to be a failure. It is undeniable, from the evidence of the Edinburgh Return quoted above, that taken through a long series of years, the rainfall apparently increases and decreases with the number of spots on the surface of the Sun ; but in the cycle of eleven years we have had under discussion, the variation in the number of spots was very exceptionally marked, and during the period exceptionally hot and exceptionally wet years occurred, yet in neither case was there any apparent connection between these abnormal conditions and the state of solar physics. It follows, then, that if it is necessary to have averages extending over half a century in order to be able to show even a slight variation of the rainfall with the number of Sunspots, and if, even then, it is possible to have an exceedingly dry year at the epoch of maximum Sunspots, it is evident that to undertake, at any particular time, important operations dependent on the weather for their success, on account of the indications of the solar cycle, would be running the risk of almost certain disappointment. Whether further investigations of astronomers will disclose other cycles than that of eleven years, it is impossible to say, but any cosmical or other changes which may assist meteorologists in their hitherto vain attempts to forecast approaching seasons will be welcomed with enthusiasm, and will be sure of an attentive and favourable investigation. That the approach of such wet seasons, as it was our fate to pass through last year, will one day be foretold in ample time to take every necessary precaution, I feel sure; but that this timely warning will not be given by considerations of the year's position in an eleven-year cycle of solar spots, I am reluctantly compelled to believe.

THE HARDENING AND TEMPERING OF STEEL.

AN announcement was made some time ago that the Institution of Mechanical Engineers had resolved to devote a sum of money to experimental research on mechanical questions. The first-fruits of this resolution have since appeared in the shape of a First Report on each of the three subjects selected by the Council for examination, viz., the hardening, tempering, and annealing of steel; the best form of riveted joints; and friction between solid bodies at high velocities.

It will be seen that these subjects are all of great importance, and that two at least—the first and the last—possess a scientific as well as a practical interest. The reports upon them do not describe any new experiments actually undertaken by the Institution. They are preliminary and historical; giving a digest of the theories that have been broached, and the experiments that have been made, by former labourers in each subject, and going on to point out wherein these are imperfect, and what remains to be done in order to achieve in each case the final solution of the problem. This historical element is itself of much interest, as showing the mode in which such questions have been attacked, both from the theoretical and the practical side; and we regret that our limits do not allow us to reproduce the reports *in extenso*. We must, however, content ourselves at present with a short *résumé* of the first subject on the list, viz., the hardening, tempering, and annealing of steel; pointing out first its essential features, and then noticing the light which is thrown upon it by the report in question.

The facts relating to this subject are extremely well known, and are continually acted upon; their importance is almost unique. Probably there is hardly a known process, scientific, surgical, mechanical, or industrial, which does not at some stage depend for its success on the use of properly tempered steel tools. It seems strange, therefore, that the theory of the subject should be in a state of the utmost uncertainty and confusion; and that even the direct experiments

made to elucidate it should have been very few. We believe we are right in saying that for some years the subject had the joint attention of two such minds as those of Faraday and James Nasmyth; but the results obtained were not considered sufficiently important to warrant their publication. It will be seen, therefore, that the question is eminently worthy of thorough scientific examination.

We will first state, as briefly as possible, the main facts of the case. We shall confine ourselves to 'hard' steel, such as is used for tools, containing not less than say $\frac{1}{2}$ per cent of carbon; with 'mild' steel, containing a smaller quantity, the phenomena are different. Hard steel, as produced mainly by the cementation process, can be hardened by being raised to a high temperature, and then suddenly cooled, or 'quenched,' in a bath of some cold fluid, generally water or oil. The outside is always the hardest, but, if the thickness be moderate, there is considerable hardening throughout. The colder the bath, the greater the hardness of the steel, until it reaches the 'ice-brook's temper' of Othello's sword; but steel in passing rapidly through so long a range of cooling is very apt to 'fly,' or become brittle. For tough steel, therefore, the range of cooling should be as low as possible. By cooling in oil, the tenacity of the steel is supposed to be increased; and this increase is believed to be greater the higher the temperature of quenching: but the difference between water-cooling and oil-cooling is probably only due to the fact that the latter is a more gradual process. Unless the initial temperature of the steel is above a certain point, the hardening does not take place at all.

It will be seen that the phenomena vary materially with the initial and final temperatures of the steel, and to some extent with the rapidity of its passage from one to the other. It will also be seen that the essential qualities of hardness and toughness are opposed to each other; in other words, the steel will be more hard, but at the same time less tough, as the fall of temperature is greater and more rapid. Hence it is of the utmost importance that the cooling should be as gradual as is consistent with giving to the steel that degree of hardness which is needful for the work it has to do; it will then be as tough as it can be made to be sufficiently hard. In the bringing of any article to this desirable condition consists the process of tempering. In practice this is effected by first heating and quenching the steel, so as to harden it in excess, then raising it again very carefully to a certain temperature, varying according to the use for which it is intended, and then quenching it again from that temperature. The manner in which the correct temperature is ascertained is very curious and striking. Take the case of an ordinary cold chisel, which is usually hardened

and tempered at one operation. The smith first heats it in his fire, protecting it from the atmosphere by laying it in the small coal. When he judges the heat sufficient, he draws it out, and dips the point in cold water, thus producing a rapid cooling and hardening. As soon as the cooling is completed, he lifts the point from the water, polishes it slightly, if necessary, with a grindstone or a file, and then watches it narrowly. The heat in the main body of the tool communicates itself to the point, and as the temperature of the bright surface rises, its original white lustre is seen to alter rapidly, first to a pale yellow, then to a straw colour, then to a full yellow, then to a brownish orange. As soon as this last tint appears, the smith drops the point again into the water, in full confidence that when cooled it will be of the right temper for its work. If, however, the heating were continued, the brown would become dappled with purple, and would then be succeeded by a full purple, light blue, full blue, and dark blue, in regular order; and each of these would mark a point at which the steel should be quenched to give the temper proper for some particular tool; *e.g.*, bright blue for swords and watch-springs, dark blue for saws.

Such being the facts, let us see how far theory has been able to account for them. This is what the Committee's Report has to tell us. Unfortunately, it is professedly deficient in its account of the German literature on the subject, owing, it is said, to the difficulty of obtaining good information from that country. This deficiency, we believe, is to be supplied in a revised edition. Meantime, we can at least deal with the French and English views of the question. We should premise that the Report has been primarily the work of Mr. W. Anderson, of the firm of Easton and Anderson, well known for his translation of Chernoff's important papers on steel; but the Committee also comprises such names as those of Chernoff himself, Professor Williamson, Professor F. A. Abel, Mr. J. Vavasseur, and others.

I. *Composition of Steel and Cast Iron.*—The early view seems to have been that the carbon in steel or cast-iron formed, at any rate in part, some definite chemical combination with the iron; but about 1852, Jullien developed the theory that the carbon was always present as a 'solution' merely—liquid when the iron was hot, solid when it was cold. The term 'solution,' as applied to a solid substance, has not been fully accepted by English chemists; but it appears to have a real signification, as expressing a mixture so intimate that the smallest particle of the mixture which can be isolated will always be found to contain both components. In some cases, as in very grey cast-iron, the carbon is not even wholly in solution, but partly exists in specks, more or less large, of pure graphite. In this state it

has generally been called 'free' carbon, and when in solution 'combined' carbon. As to the proportion in which these two forms occur in various qualities of iron and steel, Åkerman, Barba, and others, have advanced various opinions; but the view that there exists in steel or cast-iron any definite chemical combination, or carburet of iron, appears to be abandoned on all sides. It seems generally allowed that no other elements than carbon and iron should be present in really pure steel; and that other substances, such as manganese, which are generally considered to improve steel, only do so by neutralizing the action of other and more hurtful impurities.

II. *Condition of the Carbon of Iron in Steel.*—Jullien's view on this part of the subject is, that molten cast-iron is a solution of liquid carbon in liquid iron: that when the metal is cooled slowly, part of the carbon separates out as pure graphite, while the remainder continues in solution, thus forming 'grey,' or soft cast-iron; but that, when the metal is cooled quickly, this separation does not occur, and that the result is a solution of crystallized carbon in amorphous iron, forming 'white,' or hard cast-iron. Exactly the same process occurs with steel, and makes the difference between hard and mild steel. Caron and Åkerman generally confirm this view. They point out that hardened steel dissolves in hydrochloric acid without leaving any residue; but that the same steel, if first annealed, by being kept a long time at a red heat and allowed to cool slowly, leaves a residue of carbon insoluble in the acid. This seems to show that there is a more intimate mixture between the two elements in the former case, and that the carbon either dissolves with the iron, or, more probably, escapes as carburetted hydrogen.

III. *Hardening of Steel.*—On this, the central point of the whole inquiry, Jullien advances a very bold and original theory. He holds that carbon, in contact with red-hot iron, becomes liquid and is absorbed like water in a sponge; that, if cooled slowly, the carbon becomes amorphous, and the steel is soft; but if cooled quickly, the carbon crystallizes, taking the properties of diamond, and the steel becomes, in fact, diamond set in iron. The hardness of the steel is thus simply due to the hardness of the crystallized carbon. In support of this, he remarks that all hard bodies take different molecular structures, according as they are cooled rapidly or slowly; *e.g.*, gold is fibrous when cast in a metal mould, crystalline when cast in a sand mould; glass is crystalline when cooled rapidly, but amorphous when annealed. He further observes that diamond, heated for a long time in a closed vessel, becomes graphite, and hence concludes that liquid carbon, rapidly cooled, would become diamond.

This theory, striking as it is, is beset with difficulties. As

the Report observes, it is difficult to see how it can possibly account for tempering, *i.e.*, for the nice gradations of hardness, varying with each small variation of temperature, and showing each its characteristic colour, which have been described above. Moreover, as there is no known process of liquefying carbon, it is hard to believe that it is effected by the mere presence of hot air without the occurrence of any chemical reaction.

Barba and Åkerman advance a theory altogether different, viz., that the severe compression in the several layers, produced by their contraction during rapid cooling, retains a greater proportion of carbon in solution, instead of allowing it to separate out as graphite. Åkerman supposes that the compression itself makes the metal more compact, and therefore harder, exactly as it is hardened in cold-rolling and wire-drawing. But, as the Report observes, the outer layers, which cool first, are brought into a state of tension, not compression, by their efforts to contract over the still heated core; and yet it is precisely these outer layers which attain the greatest degree of hardness. There is, in fact, no reason to think that a piece of metal (*e.g.*, a thin sheet of steel) heated and then cooled uniformly, would suffer any internal compression at all; yet it would undoubtedly be hardened.

The exact molecular changes that occur during the heating and cooling of steel were ably discussed by Chernoff in 1868. His view is, that there is a certain temperature, *a*, which must be overpassed before any hardening effect can be produced by cooling; and a higher temperature, *b*, at which steel takes an amorphous, wax-like form, and on cooling from which it crystallizes into large crystals if the process is slow and undisturbed, but into small crystals if the process is rapid or disturbed by hammering. Now for toughness and uniformity, in almost any metal, fine regular grain is essential; hence steel is improved, while at the same time hardened, by heating above temperature *b*, and then cooling rapidly. If it is again heated, it begins to soften, without losing its quality; but, so long as the heat is below temperature *a*, any required degree of softness can be permanently fixed, by simply quenching the steel as soon as it has reached the proper point, as indicated by the characteristic colour. The phenomena of tempering are thus in some measure explained; but those of hardening still remain a mystery, since, although large crystals are unfavourable to the toughness, there is no reason to suppose that they affect the hardness of a metal. The existence, however, of some such molecular changes as Chernoff describes is confirmed by many other facts, *e.g.*, the sudden and temporary expansions observed during the cooling of iron wire by Gore and Norris.

Dissatisfied with the theories of their predecessors, the

Committee have boldly struck out an idea of their own. This has been suggested by Edison's experiments on platinum wire, communicated to the American Association for the Advancement of Science at Saratoga in 1879. These experiments showed that incandescent platinum wire became covered with minute fissures, due to the expiration of the occluded gases under the action of the heat: and that when the wire was allowed to cool *in vacuo*, these cracks closed up again and disappeared. By a succession of heatings and coolings *in vacuo*, the whole of the occluded gases were expelled, and the metal was then greatly altered in character, becoming much more dense and hard, and remaining perfectly rigid under the most intense incandescence. The Committee suggest that the same action may take place in steel; that the heating of the metal expels the gases which remain occluded at ordinary temperatures, and that the sudden contraction in rapid cooling prevents their re-absorption (perhaps actually assists their expulsion); the particles of the metal are thus brought closer together, and their force of cohesion is increased. When the metal is gently heated, as in tempering, re-absorption begins; and the characteristic colours are due to changes in the surface (*e.g*, the gradual opening of minute fissures) which are produced by this re-absorption.

We do not propose at present to criticize this theory. The Committee suggest the carrying out of a series of experiments, with the special view of testing its truth. But since the Report was published, the subject has been discussed at a General Meeting of the Institution, and Prof. D. E. Hughes then gave it as his opinion, supported by recent researches, that soft steel was a mere coarse mixture of iron and carbon, while in hardened steel part of the carbon at least was in the form of an actual alloy with the iron, and that the qualities of the steel depend upon the constitution of this alloy. This was illustrated by the fact that hard steel was readily attacked by dilute sulphuric acid, while soft steel was not, which he accounted for by supposing that in the former case the combination was so intimate, that local galvanic circuits were set up, each molecule forming a minute carbon and iron battery. It is to be hoped that this theory, taken in conjunction with the opposing theory of occlusion, will be fully and carefully investigated, especially as such an investigation, whether it confirms either theory or not, can hardly fail to throw considerable light upon the very interesting phenomena we have here attempted to describe.

REVIEWS.

EARLY MAN IN BRITAIN.*

THOSE of our readers who have the good or ill fortune (which is it?) of having attained middle age, will remember the pictures of the ancient inhabitants of Britain which used to adorn the first page of the *History of England.* Nearly naked figures, adorned with an elegant tattooed pattern, and bearing skins on their shoulders as their sole possible protection from the inclemency of the weather, represented our primeval progenitors; and we were quite contented with the information that they drove war-chariots with scythe-blades attached to the axles, that their priests were the Druids, who venerated the mistletoe, and under whose directions Stonehenge was built, and that, among other amiable characteristics, they had that of offering human sacrifices. At that time, and even later, the learned, although they might dispute about Picts and Scots, Celtic migrations, Scandinavian invasions, and so forth, really knew little more about the matter than the boys at school. The *History of England* began with the ancient Britons, and that was pretty nearly all that could be said.

Prof. Boyd Dawkins would carry the initial chapter of this history much further back. For him the preliminaries of human history in any given locality may be considered to range over all time, and an acquaintance with Tertiary Geology at any rate is essential to the student of man. Thus at the outset of his *Early Man in Britain,* after indicating the close interrelation of the three sciences of Geology, Archæology, and History, he sets forth the general principles of Palæontological succession, and then discusses the principal phenomena of the Tertiary epoch, the members of which he classifies and characterizes by reference to the remains of mammalia contained in the respective deposits. This, of course, is essential to the discussion of the question whether man is a Tertiary animal, into which the author enters at considerable length. His conclusion with regard to the Eocene and Miocene periods is that as none of the mammalia which lived in those times are now in existence, 'It is unreasonable to suppose that man, the most highly specialized of all, should have been then on the earth,' an argu-

* *Early Man in Britain, and his place in the Tertiary Period.* By W. Boyd Dawkins, M.A., F.R.S. 8vo. London. Macmillan and Co. 1880.

ment of considerable weight, although not quite conclusive; especially when we consider that in order to do away with certain supposed evidences of man in Miocene times, Prof. Boyd Dawkins is obliged to 'suggest that they were made by one of the higher apes then living in France rather than by man,' the things to be accounted for being chipped flints and a notched fragment of a rib of *Halitherium*. The same argument from the paucity of still extant species of mammalia is urged in opposition to the existence of man in the Pliocene; and it is not until post-Pliocene times, when living species of mammals were already abundant, that man, 'as might have been expected,' indubitably makes his appearance on the stage. From this time there is not only no doubt of the existence of man properly so called, but the persevering researches of archæologists in this country and abroad have made us acquainted with a good many particulars as to the characters and habits of these early peoples, leading gradually up to that period when documentary history of greater or less value becomes available. It is to the development of this history of prehistoric man as learned by geological and archæological research, that Prof. Dawkins' most valuable and interesting book is devoted. Its title, namely, *Early Man in Britain*, is perhaps hardly sufficient to indicate the variety and importance of its contents, for besides that it gives an excellent sketch of the European Tertiaries as elucidated by the mammalian remains contained in their deposits, even in treating of man, its more immediate object, the author's attention has by no means been confined to the narrow limits of these islands, but in investigating the sources of the human population of Britain, he has, almost perforce, entered upon the consideration of the origin and ethnological relationships of the peoples inhabiting the whole of Europe. The author's line will be best explained by the following extract from his concluding summary:—

'The River-drift man,' he says, 'first comes before us, endowed with all human attributes, and without any signs of a closer alliance to the lower animals than is presented by the savages of to-day; as a hunter, armed with rude stone implements, living, not merely in Britain, but throughout western and southern Europe, northern Africa, Asia Minor, and India. Next follows the Cave-man, possessed of better implements, and endowed with the faculty of representing animal forms with extraordinary fidelity, living in Europe, north of the Alps and Pyrenees, as far as Derbyshire, and probably belonging to the same race as the Eskimos. The disappearance of the Cave-man from Britain coincided with the geographical change from a severe to a temperate climate, the extinction of some animals, and the retreat of others to northern and to southern regions. In the pre-historic age the earliest of the present inhabitants arrived in Britain. The small, dark, non-Aryan peoples, who spread over France and Spain, brought with them into Britain the domestic animals, and the cultivated plants and seeds, and laid the foundation of our present culture. The next invaders were the bronze-using Celtic tribes, composing the van of the Aryan race. They crossed over from the Continent, and introduced a higher civilization than that of the Neolithic age. In the course of time the use of iron became known, and in the Prehistoric Iron-age the condition of Britain was higher than it had ever been before.'

This summary of the principal theme of the volume now before us will suffice to show the amount of interest attaching to its contents. The

evidence bearing upon all these points is given very fully by Prof. Dawkins, who also deals in a similar spirit with a host of correlative details, tending to show the conditions which must have surrounded our early ancestors, and influenced them in their progress towards civilization. Nor does he forget the influence exerted upon the population of Britain, and, indeed, of western and northern Europe generally, by the more advanced peoples of the Mediterranean region. Thus his book furnishes an admirable picture of the history of man in this part of the world, prior to the existence of written documents, so far as it can be made out from the occasional glimmerings of evidence which shine upon us through the twilight of the distant past. We may add, in conclusion, that this word-picture is copiously and well illustrated with pictures of another sort, which place before the student the actual representations of many of the more important objects to which the author has occasion to refer.

A TEXT-BOOK OF BOTANY.*

NUMEROUS as are the extant manuals and text-books of Botany, English students of that science certainly owe a considerable debt of gratitude to Dr. Vines for the production of a translation of Dr. Prantl's text-book. The original work was built up on the same lines as the well-known treatise of Prof. Sachs, with the purpose of furnishing a smaller and less elaborate Manual of Botany than the *Lehrhbuch*, and one which, while it might suffice for all the requirements of many students, would, at the same time, serve as an introduction to the larger work. We are not acquainted with the German original, but, so far as we can judge, the translation has been very carefully made, and the treatment of the subject is so concise and simple that the book cannot but exercise a most beneficial influence on those who use it as a guide in their botanical studies.

The author commences with a short chapter on the general morphology of plants, and then, in his two succeeding sections, treats of their minute anatomy and physiology. The special morphology of the various types of plants is relegated to the sections treating of classification, in which the structure and relations of the different parts of plants are described in some detail under the heads of the great divisions of the vegetable kingdom. This arrangement of the subject is very advantageous, and leads the student directly to a more philosophical conception of the subject than was generally attainable from the older manuals.

The section on classification, enlarged as above indicated, occupies more than two-thirds of the volume. The classification adopted differs in some particulars from the ordinary one in use in this country, especially in the wider limits of the orders adopted, which are, consequently, reduced in number, while some groups of plants are referred to somewhat different positions from those usually assigned to them by English writers. The translator has,

* *An Elementary Text-Book of Botany.*—Translated from the German of Dr. K. Prantl, Professor of Botany in the Royal Academy of Forestry, Aschaffenburg, Bavaria. The Translation revised by S. H. Vines, M.A., D.Sc., F.L.S. 8vo. London: Sonnenschein & Allen, 1880.

however, given a consensus of the two classifications of Phanerogams, which will be of material service to the student. He has also substituted Prof. Sachs' classification of Thallophytes for that given by the author. We notice that the composite Algo-fungal nature of the Lichens is here accepted as proved. The woodcut illustrations are excellent throughout the whole book.

ZOOLOGICAL CLASSIFICATION.*

WE are very glad to see that Mr. Pascoe's little handbook of systematic Zoology, the first appearance of which we noticed in April, 1877, has been so successful as to cause him to bring out a second and greatly enlarged edition of it. The general plan of the work continues the same, but the author has modified the classification here and there in accordance with treatises published during the last four years, and he has added greatly to the statements of the opinions of different authors on the classification and value of various groups, thus making his book, which is still small enough to be a most convenient pocket-companion, an exceedingly valuable compendium of Zoology. The glossary of zoological, anatomical, and physiological terms, which the author has appended to the book, and which, in general, will be found useful, is certainly in some points not so good as it ought to be, some of the definitions being unsatisfactory, or even absolutely erroneous.

In all other respects we can only endorse the opinions expressed in our former notice of this book. The author has evidently bestowed immense labour on its preparation, and while we admit that there are portions of the work which are fairly open to criticism, it would be ungracious to point out small defects when the general result is so excellent.

THE MOUND-BUILDERS.†

MUCH has been written by American archæologists on the subject of those mysterious earth-mounds which are abundantly scattered through the valleys of the Mississippi and the Ohio. Mr. MacLean has done useful work in concentrating the literature of the mounds so as to bring its essence within compass of a small octavo volume. Those readers who have not the opportunity of procuring, or the time for reading, the larger works on American archæology, will find in this little book a popular summary of what is known about the mounds and their contents. The implements in stone and

* *Zoological Classification: a Handy Book of Reference, with tables of the Sub-kingdoms, Classes, Orders, &c., of the Animal Kingdom, their Characters, and Lists of the Families and principal Genera.*—By Francis P. Pascoe, F.L.S. Second edition, with additions and a Glossary. Sm. 8vo. London: Van Voorst. 1880.

† *The Mound-Builders, being an account of a remarkable people that once inhabited the Valleys of the Ohio and Mississippi; together with an investigation into the Archæology of Butler County, Ohio.* By J. P. MacLean. 8vo. Cincinnati: Robert Clarke & Co. London: Crosby, Lockwood & Co. 1879.

copper, handmade pottery, the quaintly wrought pipes, the great plates of mica and lumps of galena, brought from a distance and deposited in the mounds, are relics which tell of a primitive people dwelling in the country before the Red Man got possession of the land; but who these mound-builders were, whether Nahoans or Toltecs, or what not, no man at this day can confidently assert. Notwithstanding the multitude of relics which have been unearthed, we may still say of this subject, as old Camden said of quite another matter, 'we walke in a mirke and mistie night of ignorance.' It should be remarked that Mr. MacLean not only gives a general account of the mounds, but includes in his book an original archæological essay on the mounds of Butler County, in Ohio.

ELECTRIC LIGHT.*

THE recent popularity of the Electric Light seems to have produced a large crop of literature on the subject, of very unequal value and character. The present work is stated in the Preface 'to contain a general account of the means adopted in producing Electric Light.' 'No attempt has been made to teach the science of electricity.' To this modest undertaking of Mr. Urquhart, Mr. Webb adds a prefatory note to the effect that 'in revising for the press Mr. Urquhart's little work, he has endeavoured to arrange the matter in accordance with the history of the subject, and to make a few additions on historical, theoretical, and experimental points.' Beyond these statements, it is not very clear in what proportion the two writers named on the title-page have severally contributed to the general result. A short introduction leads to a chapter on voltaic batteries, very trivial in character, at times incorrect, as, for instance, in stating that 'the dense variety of carbon known as graphite, found in gas retorts, is of little use;' and again, that 'it is better to give the carbons in Bunsen batteries a heading of lead,' and to solder a piece of copper to the top of the platinum for a Grove's battery. The author's modification of 'Burnes' negative plate cell' is stated to be 'very portable, and may be made use of in travelling, to secure photographs of caves and such places.' The negative consists of a plate of copper, to one surface of which, as well as to its edges, a sheet of platinum foil, compact and free from pinholes, is soldered, and to the opposite surface, or back, a sheet of lead.' The exciting solution used consists of sulphuric acid and bichromate of potash, substances calculated to do no little havoc on this singular compound of platinum, lead, copper, and soft solder. Air is moreover pumped into the cells by means of leaden pipes, inserted in the liquid! Ten of these batteries are stated 'to produce the effects of sixty or seventy Grove or Bunsen cells!' 'To prevent the possibility of any disappointment in the use of this apparatus, it will be as well to tell the reader at once, that for every fifteen minutes or so of Electric Light, the cells will be nearly exhausted, and to continue at full power, ought to be refilled.'

* *Electric Light; its Products and Use, &c.* By J. W. Urquhart, C.E. Edited by F. C. Webb, M.I.C.E., M.S.T.E., with ninety-four illustrations. Small 8vo. London: Crosby, Lockwood and Co. 1880.

Chapters written in the same slipshod style follow, on Thermo-electric batteries (5 pages), magneto-electric generators, electro-magneto-electric machines (6 pages), dynamo-electric machines, evidently gathered from familiar sources, containing nothing new except what is not true; and so at the 150th page we arrive at some 'general observations on machines.' Here, at last, we meet with traces of practical, if not of theoretical knowledge, such as recommendations 'to start the machine with open circuit,' and 'never to place the machine at full speed on short circuit,' to drive steadily, and to use sperm oil for lubrication. 'A machine that heats much is not properly constructed, and may be improved by taking a layer of wire off the electro-magnet.' Chapter VIII. deals after the same fashion with 'electric lamps and candles,' naming those best known, and omitting, as usual, their common ancestor, the Chapman lamp, already figured in this journal. The author contributes a lamp about as good as his battery. Of this invention it is naively said, 'When the lamp has given light for about six hours or more, and it is required to continue for another six hours, it is only necessary to slightly lower the plates by the screws in the armatures. *In practice this cannot be done just as directed.*'

Mr. Edison is dismissed very cavalierly. 'Much interest,' says our author, 'has been taken in the sensational and often absurd announcements concerning the apparatus in course of perfection by Mr. T. A. Edison, of Menlo Park, New York. This inventor is well known by his talking phonograph (*sic*) and telephones, and it was in some quarters thought that when he had set himself to the task of inventing an efficient subdivision of the electric light circuit, something would in all probability be done. Unfortunately, however, as far as can be learned up to this date (July, 1879), the attempts have proved almost complete failures.'

In Chapter IX. we have nine pages on the measurement of the electric light, in which Rumford's and Bunsen's photometers are very feebly described, followed in Chapter X. by eleven pages on the 'mathematical and experimental (*sic*) treatment of the subject.' Hopkinson, Schwendler, Preece, and Siemens, are laid under contribution, but to very little purpose. The book concludes with about a dozen pages on the cost of the light. The quantity of information given in 290 pages is infinitesimally small.

Messrs. Crosby and Lockwood deserve much credit for the getting up of the book. The print is excellent, the paper beautiful, the woodcuts superb. Their motto on the title-page is *Capio lumen*—it would have been better in this case had they modified the first word to *Rejicio*. W. H. STONE.

BRITISH COPEPODA.*

IN the course of the thirty-six years of its existence the Ray Society has certainly well fulfilled the purpose for which it was established. A great number of copiously, and often most beautifully, illustrated monographs

* *A Monograph of the Free and Semi-parasitic Copepoda of the British Islands.* By G. Stewardson Brady, M.D., F.L.S. 2 vols. 8vo. London: Ray Society. 1878-1880.

have been issued to the subscribers; works which it would have been impossible for any publisher to bring out in the same style without certain loss; and we are glad to take the opportunity of the appearance of the second volume of Mr. Brady's book on the British Free and Semi-parasitic Copepoda to remind our readers of the existence and activity of this Society, which certainly merits a larger share of patronage than would appear to fall to its lot. This is not the place to give a list of its publications, but we may cite the magnificent work of Alder and Hancock on the Nudibranchiate Mollusca, Forbes' Naked-eyed Medusæ, Professor Allman's Freshwater Polyzoa, Mr. Blackwall's British Spiders, and Mr. Darwin's Monograph of the Cirripedes, as showing how much naturalists are already indebted to the Ray Society.

Among the earlier volumes published by the Society was Baird's *Natural History of the British Entomostraca*, an excellent and standard work, founded to so great an extent upon the author's personal investigations that although it is now somewhat out of date it must always hold its position as a book of reference. It was published in 1850; and the best proof of the vast strides that have been made in this department of Zoology, of late years is to be found in the fact that Mr. Brady has now devoted two octavo volumes to the description of the members of only one of the six orders recognized by Baird. The truly parasitic forms, whose alliance with the crustaceans described by Mr. Brady is, in many cases, established only by the investigation of their development, were placed by Baird in a separate division of the Entomostraca; and this distinction is still maintained by some high authorities, so that his Copepoda include precisely the free-swimming and semi-parasitic (or commensal) forms which form the subject of these volumes.

Of his Copepoda, Baird describes and figures only thirteen species, but in the case of the well-known freshwater *Cyclops* he cites three varietal forms, so that we may reckon that he recognized fifteen forms in the order. Mr. Brady, however, describes fourteen species of the genus *Cyclops*, as now restricted, whilst the total number of British Copepods with which he expects to have to deal in the execution of his present task, amounts to at least 151. Of these, 134 are treated of in the volumes before us, the remainder, consisting of three families of somewhat aberrant and semi-parasitic forms, will form a third volume, which will also contain a general sketch of the anatomy and physiology, and development of these organisms. In his Introduction, Mr. Brady gives short directions as to the best modes of collecting, examining, and preserving the Copepod Crustacea.

Of the systematic part of the work, considering the reputation of the author, it is scarcely necessary to say much. As we have already remarked, he differs somewhat from certain other writers in his views as to the affinities of the free and parasitic Copepods, and this, to a certain extent, influences his opinions as to the classification of the former; but only with regard to certain doubtful forms, as to which an absolute agreement between different geologists is scarcely to be hoped for. In every respect the work is most carefully executed, and it cannot fail to prove of the greatest advantage to the students of our British fauna. The plates, of which there are in all eighty-two, furnish outlines, and sometimes coloured figures of the species, with copious illustrations of the structure of those parts which are

of most systematic importance. These figures have been drawn by the author himself, and carefully lithographed by Mr. Hollick. They, of course, add immensely to the value of the work.

THE GEOLOGICAL RECORD.*

WE are exceedingly glad to be able to call attention to the publication of another volume of this valuable guide to contemporary geological literature. The present issue has been, indeed, a little tardy in making its appearance; but the editor informs us in his preface that this is due to the temporary loss of the MS. of the section on European Geology, which, he says, was 'appropriated by some very wise person as an article of great value, and not recovered for some months.' The general arrangement of the contents is the same as in the last volume, but we observe that the editor has adopted an approximation to the plan which we ventured to suggest in noticing the latter, by placing the supplementary notices relating to former years as appendices to the sections to which they belong, instead of bringing them all together in a miscellaneous mass at the end of the book, where they stood in much danger of being overlooked. This is, undoubtedly, a great improvement, but we still think it would be better to do away with supplements altogether, and work their contents into the body of each section, merely distinguishing the supplementary articles by the addition of their dates. Mr. Whitaker does not claim perfection for the work offered to their *confrères* by himself and his able coadjutors; indeed, he insinuates that the attainment of perfection might be unsatisfactory to some people who are fond of 'discoursing eloquently of "the imperfections of the *Geological Record*;"' but we are thankful to say, that the fear of such a catastrophe does not influence these 'Able Editors,' who evidently aim at making their Records of Geological Literature as perfect and useful as possible, and certainly with no stinted measure of success.

NEW ZEALAND ZOOLOGY.†

PROF. HUTTON'S '*Zoological Exercises*' is the outcome of his adoption of the method of teaching Natural History advocated and illustrated by Professors Huxley and Martin in their *Elementary Biology*. His students required a handbook, and that just mentioned, from its dealing only with British organisms, and for some other reasons, did not quite suit the wants of the New Zealand Professor, who, accordingly, set himself the task of pre-

* *The Geological Record for* 1877. *An account of Works on Geology, Mineralogy, and Palæontology, published during the year.* With Supplements for 1874-76. Edited by William Whitaker, B.A., F.G.S. 8vo. London: Taylor & Francis, 1880.

† *Zoological Exercises for Students in New Zealand.* By F. W. Hutton. Sm. 8vo. Dunedin: J. Wilkie & Co., 1880.

paring a manual which should deal with characteristic types of animals inhabiting New Zealand, more or less after the same fashion.

It will be seen that in one important respect Prof. Hutton departs from the method advocated by Prof. Huxley, namely, that of regarding Zoology and Botany as forming one single science; further, while noticing a much larger number of animal types, his investigation of them is by no means so elaborate as that sketched in the English book; but on the other hand, his little work includes certain elements wanting in the latter, and which will, we think, peculiarly adapt it to be useful to those who are seeking to instruct themselves in isolated and out-of-the-way places. These are a description of the microscope, and a guide to its use, preceding the section on Morphology; and a general synopsis of the classification of animals, and a tabular analysis of the families of insects which follow it. The last-mentioned portion of the contents is intended to enable students to make their first steps in taxonomical zoology. The author wishes his readers to collect numerous insects, and then, by means of this table, to refer them to the families to which they belong.

PONDS AND DITCHES.*

WE welcome this little volume on account of its intrinsic qualities, and also as it may serve to call the attention of a considerable number of readers to the interesting and instructive series of natural history objects that may be obtained from sources that most people pass, either without consideration or with a feeling of contempt. The author has given a good sketch of the botany and zoology of ponds and ditches, indicating, generally, the more striking and interesting members of the various groups of plants and animals that may be found in such localities. So far as it goes his work is a very satisfactory one throughout, and but for two or three objections it might justly have been recommended as a most satisfactory guide to that wealth of organisms that people even the most insignificant pieces of standing water. The author commences by describing the principal flowering plants which grow either in the water of ponds and ditches, or on their margins, then notices, in their order, the lower forms of vegetable existence, the freshwater Algæ, Desmids, and Diatoms, and finally the animals which abound in such places, commencing the latter part of his work with the Protozoa, and working upwards to the insects and their larvæ. But, unfortunately, by what seems to us a great error of judgment, he has omitted all notice of the pond-haunting fishes and Batrachia, which would furnish materials for two interesting chapters, and this because they have already been treated in another volume belonging to the same series. We presume it is for the same reason that the pond Mollusca are also passed over *sub silentio*, and these two omissions seem to us to detract immensely from the value of Dr. Cooke's little volume, which might so easily have been made a very excellent guide to the

* *Natural History Rambles.—Ponds and Ditches.* By M. C. Cooke, M.A., LL.D. Sm. 8vo. London: Society for Promoting Christian Knowledge, 1880.

mysteries of pond-life. This incompleteness is the more to be regretted as the greater part of the remainder seems to be very carefully prepared, the only chapter that shows striking indications of weakness being that on aquatic insects, in which no mention is made of several forms common in ponds. We may especially notice the curious larvæ of *Stratiomys* and *Eristalis* as being entirely unmentioned, as also the Palpicorn water-beetles, including the great *Hydroüs*, which was certainly worthy of a few words. We notice, further, that the larva of *Acilius sulcatus* is figured as if it was that of *Dyticus*; and that the author's notions of the aquatic Hemiptera are rather confused. The little book is well illustrated with woodcuts. It concludes with a chapter on collecting and examining specimens, and notwithstanding the defects which we have noticed above, it will undoubtedly prove a useful companion to the young naturalist in his first steps towards the knowledge of freshwater organisms.

BRITISH MOSSES.*

MOSSES, perhaps, receive about as little attention from Botanists as any class of plants, and considering how many Botanists are mere collectors, and how admirably mosses lend themselves to the collector's purposes, this is rather remarkable. Something may be due to the minuteness of the size of many of the species, and something perhaps to difficulties inherent in the systematic treatment of these plants; but we fancy the chief cause of the comparative neglect with which they are treated is to be sought in the want of a good illustrated English treatise upon them. The flowering plants and ferns are enthusiastically studied by many; sea-weeds are not wholly neglected; the study of the fungi may almost be regarded as a prevalent fashion, and the lichens have their admirers; but upon all these groups the British Botanist possesses more or less reliable guides in his own language.

In the work of which the first part is now before us, Dr. Braithwaite aims at placing the British Mosses on the same vantage-ground as the more favoured classes of the Vegetable Kingdom; and judging from the sample lately issued, he will succeed in his endeavours. In this first part, indeed, he has not got very far with his task—it relates solely to that curious order of mosses, the Andreæaceæ, which differ from all other members of the class, and resemble the Jungemannieæ, in the longitudinal splitting of their capsules, but show their closer affinity to the true mosses, in the general structure of the plants, the presence of a columella in the fruit, and the absence of elaters. Of these plants, which grow always upon rocks on high mountains, extending up to the borders of the perpetual snow, the author distinguishes five British species, which he describes fully, indicating and discussing their synonymy in detail, and illustrating their structural peculiarities by numerous figures drawn by himself, and forming two plates.

The work, if carried on as it has been commenced, must attain con-

* *The British Moss-Flora.* By R. Braithwaite, M.D., F.L.S. Part I. 8vo. London: Published by the Author (at 303 Clapham Road). 1880.

siderable dimensions, but it bids fair to furnish British Botanists with a very complete Hand-book of the Mosses, embodying the results of recent foreign work, even to the extent of indicating the extra-British European species. Our only regret is that the author should have adopted the very inconvenient system of personal publication, which always increases the difficulty of procuring a book.

SPRING.*

IT is rather remarkable in these years to find any one capable of getting enthusiastic on the subject of Spring. Of course there are many effete articles of faith to which some men cling tenaciously for years after they have been proved to have no real foundation; but these generally rested originally upon intangible bases; and it is well known that many people (especially women) persist in the belief that certain other persons are angels, whom the general voice of society would pronounce to be of a very different quality. Such delusions must be matters of memory, and to the same category we must refer Mr. Heath's encomiums on Spring. For the last few years, indeed, it has been rather hard to say when spring began or ended. The whole year has been one continuous wet March, varied only by frost and snow and fogs, in those months which have always been denominated winter, and by occasional hot days in the season which is still, by courtesy, named summer; and it can only be by an effort of memory that even a poet could sing the charms of spring. To avoid confusion (influenced probably by the peculiar meteorological conditions above referred to), Mr. Heath makes spring commence in January. He might also, with some show of justice, have made it include June, but, yielding to old prejudices, he contents himself with May.

The earlier chapters or sections of Mr. Heath's *Sylvan Spring* contain general descriptions of the delights of country, and particularly of woodland scenery in the early part of the year, and this part of his work especially we must regard as embodying reminiscences of the past and anticipations of the future rather than the records of present experiences. The sun shines brightly in Mr. Heath's pages; the flowers bloom, the birds sing, and the trees burst into leaf as they used to do in days long past, and the whole story of Spring is told with a most enviable freshness. In the later sections our author ties himself down to a chronological arrangement of his subject, and treats successively of the months from January to May, indicating under each the principal phenomena which may be observed in the progress of vegetation, and interspersing his narrative with some notices of birds and insects, among the latter, especially, the butterflies. The book is illustrated with numerous woodcuts, among which some of those representing country scenes are exceedingly beautiful; and also with a dozen plates printed in colours, which, although not first-rate productions, are pretty and attractive

* *Sylvan Spring.* By Francis George Heath. Sm. 8vo. London: Sampson Low & Co., 1880.

enough. Like all Mr. Heath's works, this seems to be well adapted to its purpose of exciting a love of nature and a desire for studying natural objects in its readers' minds, and few books could be more appropriately placed in the hands of young people who have the good fortune to be able to pass any portion of the earlier months of the year among the rural scenes which it so successfully depicts.

GEOLOGY OF THE LONDON DISTRICT.*

WE are glad to be able to announce the publication of a third edition of Mr. Whitaker's memoir on the Geology of the London district, the explanation of the Geological Survey map of that area, and of the geological model of London, which may be seen in the Museum of Practical Geology in Jermyn Street. The book, as we stated in noticing its first issue, affords an excellent summary of the geological phenomena observable in London and its immediate neighbourhood, and Mr. Whitaker has carefully worked into it the results of recent investigations, some of which are of considerable interest and importance. The most important of all the additions are those relating to the demonstration of the existence of a great ridge of Palæozoic rocks under the London district, upon which so much light has lately been thrown by deep borings in quest of a supply of water. All these are noticed in some detail, and at p. 19 there is a very useful table showing their depths and the various formations passed through in sinking them.

* *Guide to the Geology of London and the neighbourhood.* By William Whitaker, B.A., F.G.S. Third edition, 8vo. London: Longmans and Stanford, 1880.

SCIENTIFIC SUMMARY.

ASTRONOMY.

Solar Parallax.—Mr. David Gill, her Majesty's Astronomer at the Cape of Good Hope, has at length completed his investigation of the value of the solar parallax, and he communicates the result to the Royal Astronomical Society. It will be remembered that Mr. Gill went on an expedition to Ascension Island in the year 1877 in order to determine the solar parallax by measuring with a heliometer the distance and position of the planet *Mars* from certain stars in the evening and morning. The expedition proved completely successful. Since this period Mr. Gill has been engaged in reducing these measures with the greatest care, taking every precaution to eliminate every source of systematic error. On twenty-five occasions he was able to obtain a complete series of measures of the distances of the planet *Mars* from the same star in both the evening and morning, this observation being superior to those in which the position of the planet had to be measured from different stars in the evening and morning. These twenty-five sets of measures give for the value of the solar parallax

$$\pi = 8''{\cdot}780 \pm 0''{\cdot}013$$

When all the observations are reduced together, the result is

$$\pi = 8''{\cdot}778 \pm 0''{\cdot}026$$

The larger probable error shows that this result is inferior in accuracy to the other.

The separate results range between $8''{\cdot}62$ and $8''{\cdot}90$, so that they are fairly accordant, and would seem to render it probable that the resulting value,

$$\pi = 8''{\cdot}780,$$

is not so very far from the truth. This value is considerably smaller than is generally believed to be correct, most astronomers considering that the real value is not very far from

$$\pi = 8''{\cdot}84$$

The result of the last transit of *Venus* has given all kinds of value, from $8''{\cdot}76$ to $8''{\cdot}90$, and up to the present time no very satisfactory result has been obtained.

Lately an attempt has been made by Messrs. Campbell and Neison to determine the value of the solar parallax from the parallactic inequality in

the motion of the Moon by taking means to eliminate all the systematic errors which have hitherto rendered discordant the results obtained by this method. The method of determining the solar parallax by means of the parallactic inequality would be, perhaps, the most favourable of all were it found possible to eliminate these systematic errors. This Messrs. Campbell and Neison have undertaken to do. The value they obtain for the solar parallax is

$$\pi = 8''{\cdot}778 \pm 0''{\cdot}007$$

This value is conditional, however, on the assumption that there is a periodical variation in the motion of the Moon due to the action of the planets, which systematically affect the value found for the parallactic inequality. The fuller inquiry whether there really exist such periodical variations, the authors defer to a subsequent research. If there do not exist such variations, then they show that the parallax of the Sun is

$$\pi = 8''{\cdot}848 \pm 0''{\cdot}007$$

Transit of Venus on December 6, 1882.—The Astronomer Royal has lately communicated to the Royal Astronomical Society an account of his proposed plans for observing the transit of *Venus* on December 6, 1882. The method to be used is Delisle's, which consists in observing the exact time of the ingress or egress of the planet from points on the surface of the earth where this ingress or egress is much retarded or accelerated.

For places where the ingress of the planet on the Sun will be much accelerated, it is proposed to use stations in the Cape Colony, from the observatory at Cape Town to D'Urban.

For stations where the ingress of the planet on the Sun will be much retarded, it is proposed to utilize places in the West Indian Islands, and especially Barbadoes, Trinidad, and Jamaica.

For places where the egress of the planet from the Sun will be much accelerated, it is proposed to use the same stations as those for retarded ingress.

For stations where the egress of the planet from the Sun will be much retarded, it is proposed to use places on the eastern coast of Australia, including Melbourne and Sydney, and, if possible, New Zealand.

It is proposed to entirely give up the use of photography, on which so much reliance was placed by Sir G. Airy and his advisers prior to 1874, but which the results of that transit showed to be useless when employed in the manner it was.

It may be remarked that the commencement of the transit will be visible in England, commencing shortly before sunset.

Ultra-Neptunian Planets.—For many years astronomers have speculated on the probability of there being several planets belonging to the solar system and revolving in orbits beyond that of *Neptune.* Such a planet would probably be so faint and would move so slowly that it might easily escape detection for many years. There is, moreover, a peculiar relationship between the aphelion distances of a number of periodical comets and the mean distance of the planet *Jupiter,* which has led astronomers to suppose that these comets have been introduced into the solar system by the attraction of that giant planet. There are other comets whose aphelion

distances are similarly related to the planet *Neptune*, and might be supposed to have been introduced into the solar system by the attraction of that planet. For this reason it has frequently been pointed out that there may be ultra-Neptunian planets with mean distances of about 45 and 75 times that of the Earth, as there seems to be a tendency for periodical comets to have their aphelion distances at about one of these distances from the Sun. Nothing beyond mere speculation seems to have engaged the attention of astronomers. At the end of April, however, Prof. George Forbes published the following definite statement:—'Prof. G. Forbes, of Anderson's University of Glasgow, has reason, from the study of the action of the planets on comets, to believe that a planet exists at present in right ascension $11^h\ 40^m$ and North Polar distance 87", or within, perhaps, 5° or so of that spot, close to the ecliptic; that the opposition is just past, and that its distance from the Sun is 100 times that of the Earth.'

Much interest was aroused by this statement, and much curiosity excited as to the means by which Prof. Forbes had arrived at so definite a prediction of the position of so distant a planet. It was obvious that he could not have made use of a method of inverse perturbations, like the method employed by Adams and Leverrier for the discovery of the position of *Neptune*, for, apart from other considerations, this would require his proving that there was no other planet between *Neptune* and this new one, which may be termed Forbes' planet, and this is impracticable with our present knowledge.

It was evident, therefore, that he must have employed some method based on the position and dimensions of the orbits of the known periodical comets. It was by no means obvious, however, how from these data there could be determined the present position of the planet, even granting that it might be legitimately assumed that such a planet had introduced certain comets into the solar system.

Lately Prof. Forbes has published some details of his method of investigation. Prof. Forbes assumes, firstly, that any comet with a given aphelion distance has been introduced into the solar system by a planet with a mean distance sensibly equal to the aphelion distance of the comet; and secondly, that this comet was introduced into the solar system at a time when the comet was at its aphelion and when the longitude of the planet was nearly coincident with the longitude of the aphelion of the comet. Both of these are fair assumptions to work on. He shows that there is a group of comets with period ranging between 300 and 500 years, which have their aphelion distance at about 100 times the mean distance of the Earth. In accordance with the basis of his investigation he assumes that these comets have been introduced into the solar system by the attraction of a planet whose mean distance is about 100 times that of the Earth, and whose period of revolution around the Sun is about 1000 years.

Prof. Forbes then inquires what must be the position of this planet at the present time, so that by its motion in past ages this planet would have been in the right position to introduce these comets into the solar system. In a highly ingenious manner Prof. Forbes shows that if the present longitude of the planet be about 180° and it moves about $\frac{1}{3}$° per year, it would have been in past ages in the right position to introduce into the solar system the greater number of the comets in this group. Thus, in about the year 1650

it might have introduced the comet I. of 1843, and in the year 1608 have just moved into the right position to introduce comet II. of 1850. Still earlier, in 968, it would have been in the position to introduce comet IV. of 1840, and in the year 409 the comet I. of 1861. Assuming, then, the planet did introduce these comets at these times, Prof. Forbes is able to show that the planet must have a period of about 1006 years, a mean distance of $100\frac{1}{3}$, and be in longitude 174° in the year 1880. Consequently its present position would be Right Ascension 11^h 40^m, and North Polar Distance 87°. Prof. Forbes then inquires if there be any stars now missing from the Catalogues which may be really observations of this planet. The only one he finds is star No. 894 of the *Greenwich First Seven-Year Catalogue*. This is a star which was observed twice in the year 1857, but on no subsequent occasion, and its magnitude is not stated, but cannot have been less than eighth or ninth magnitude. It is not unlikely that this supposed star may have had no existence, and the observation have been that of some known star, written down with a wrong reading, or it may have been a faint star observed by mistake, having been taken for a minor planet, which would account for its not being observed again; but, as Prof. Forbes points out, it may have been his planet, which would have been quite close to the place at the time.

If this planet really exists, what would be its probable dimensions and appearance? *Uranus* and *Neptune* are both about 35,000 miles in diameter, and it may be assumed that the new planet is of the same dimension. Then, from its great distance, it would present a disc only 0″·8 in diameter, or practically undistinguishable in size from that of a star, unless it be with a very large and perfect instrument. Its brightness would also be much less than that of *Neptune*, as not only would it receive but one tenth of the amount of light, but there would be only one-tenth as much reflected to the Earth, so that it would shine with only one-hundredth of the brightness. The brightness of *Neptune* is that of a star of the eighth magnitude, so that the brightness of the new planet would be only that of a star of the fourteenth magnitude. Its mean daily heliocentric motion would be $3\frac{1}{3}''$ of arc, which would be so slow that it could only be detected after several days' interval. Considering, therefore, the very great number of fourteenth-magnitude stars which there are in any small area in this portion of the heavens, probably many hundreds in the area which must be searched to find this planet, and the necessity that there would be of mapping the whole district as practically the only way of detecting so slow a motion, it may be regarded as almost hopeless to search for such a body. It is probable that the planet would not even be visible with less aperture than 9 or 10 inches, and it would require a telescope of at least 15 or 16 inches aperture to properly search for it. Unless, therefore, this new planet of Forbes is much larger than either *Neptune* or *Uranus*, its discovery is well-nigh hopeless.

But if the missing star from the *Greenwich First Seven-Year Catalogue* be really this new planet, it must shine like a star of the eighth or ninth magnitude. To do this it must have a diameter at least eight times that of *Neptune* or *Uranus*. This would be excessively improbable, for it would imply a diameter of 280,000 miles, or three times that of *Jupiter* and nearly one third that of the Sun, whilst its mass would probably be nearly twenty times that of *Jupiter* or one-fiftieth of that of the Sun, assuming

the mean density of the planet to be less than that of any other. Its apparent diameter would be over 6″, so that it would appear like an exceedingly bright planetary nebula, and could not well be mistaken for a star by the Greenwich observers. Moreover, it would certainly produce well-marked perturbations in the motion of the greater planets. This consideration seems to render it out of the question that the missing star from the Greenwich Catalogue can be Forbes' planet. For it to be really rendered possible for this star to be Forbes' planet, the planet must be supposed to shine very brightly by its own light, and to be of nearly the dimensions of the planet *Jupiter*. It would then shine like a ninth-magnitude star, and might be detected without very great difficulty. The assumption, however, that a planet of this size could be shining brightly by its own light, is one which is far from being probable. Even under the most favourable circumstances it is not likely that an Ultra-Neptunian planet would possess greater dimensions than the planet *Jupiter*. This would render its apparent diameter about equal to that of *Neptune*, and its brightness to that of a star of the eleventh magnitude. The planet's motion being so slow, it would not be an easy object to detect, but it is not improbable that a long search with a good instrument over an area of 5° by 2° would enable such a body to be detected if it existed.

From the above consideration it would appear, therefore, that even if Prof. Forbes' planet really does exist, its detection cannot be an easy matter.

On a Suspected Variation in the Position of the Earth's Axis.—It has been pointed out, that if from any cause the axis of rotation of the Earth does not coincide with the axis of figure, the former will revolve round the latter in a period of about 306 days. Accordingly there ought to appear a small variation in the apparent latitude of Greenwich, deduced from observation of the Pole-star, which would go through all its variations in about ten months. Professor Clerk Maxwell endeavoured to ascertain whether the *Greenwich Observations* for the years 1851–54 gave any indication of the existence of such a variation, but with doubtful results. The subject has been lately taken up by Mr. A. Downing, of the Royal Observatory, who has discussed the observations of *Polaris* made between 1868 and 1877. He finds evidence of a small periodical variation with a ten months' period. The co-efficient of this term amounts, however, to only 0″·075 ± 0″·15.

This would indicate that the axis of figure of the Earth revolved round the axis of rotation in a circle, about six feet in radius, once in ten months. This result is in close accordance with one derived by Professor Peters from a discussion of the observations made at Pulkowa. On the other hand, Dr. Nyrén has arrived at values for this variation in latitude at these different periods, which are discordant with each other, and would indicate some different source as that of the variation.

Winnecke's Comet.—In the *Astronomischen Nachrichten*, No. 2014, there is an important note by Prof. von Oppolzer giving some of the results of his recent researches on the motion of Winnecke's Comet. This is a well-known comet, of short period, and has been carefully observed during its appearances in 1858, 1867, and 1875. From his investigation of the perturbations in the motion of the comet, Prof. von Oppolzer shows that the observations made at these

three appearances cannot be brought into agreement unless it be assumed, (1) that the mass of the planet *Jupiter* must be reduced from Bessel's value 1 ÷ 1048 to 1 ÷ 1051; or (2) that the comet suffers a similar retardation in its motion to that which Prof. Encke pointed out was indicated by the motion of Encke's comet. The former of these two alternatives seems out of the question, as all recent investigation of the mass of *Jupiter* from the motion of its satellites, as well as Prof. Axel Möller's researches on the motion of Faye's Comet, and Prof. Kruger's investigation of the perturbation of the minor planet *Themis*, agree in confirming Bessel's value for the mass of *Jupiter*. It would appear, therefore, that Winnecke's Comet really experiences a similar retardation of its motion to that which seems to exist in the case of Encke's Comet; this retardation, as it is well known, by diminishing the dimensions of the orbit shortens its period of revolution. Accordingly Prof. von Oppolzer finds, after each appearance, a mean daily acceleration in its sidereal motion of 0″·01439. This, as Prof. Oppolzer shows, corresponds to a value of the retardation in close agreement with that found by Encke from the motion of the famous comet bearing his name. Of late years the existence of this resisting medium has been considered doubtful, for Encke's Comet seems to be the only one showing any well-defined trace of its effects. Prof. Axel Möller had most carefully investigated the motion of Faye's Comet, another of short period, but failed to find any trace of such retardation. Prof. Oppolzer is of opinion that this is not surprising, as the effect of such a resisting medium on Faye's Comet would be so small that its effects could not be disentangled from the effects of the unknown errors in the perturbations.

The question is one of very great interest, and it is very desirable that as much light as possible should be thrown on the subject. It requires a rigid and exhaustive investigation of the motion of all the comets of short period—a work of very great labour. It is probably only in this manner that further light can be thrown on the subject.

The Great Southern Comet, 1880.—From the further particulars which have now reached England, this comet seems to have been a bright comet of considerable size, and if visible, under more favourable conditions, would have been probably of great brilliancy. It moved in an orbit which approached very close to the Sun, so that the comet swept round its perihelion passage with enormous velocity, and rapidly rushed off into space. It was not seen until several days after perihelion passage, when its tail was observed as a bright band rising above the western horizon. The comet was moving tail foremost, and the nucleus could not be seen; and even the tail, which was of great length, was only visible in bright twilight. The comet grew rapidly fainter, and as it receded from the Sun the nucleus could be detected as a small bright mass. Long before the end of the month, however, the comet had become so faint that it could no longer be observed, even with the great Melbourne reflector. It is probable, that under more favourable circumstances, this comet would have taken rank as one of the largest and most brilliant comets of the century. It never rose sufficiently above the horizon for even its tail to be visible in England.

Immediately on the arrival of the observations in Europe, the calculation of the orbit of the comet was undertaken by astronomers, and especially by

Mr. Hind and Prof. Weiss. It was found to present a very great resemblance to the orbit of the Great Comet of 1843: a resemblance so close, that it seems almost certain that they must belong to the same body, that is, the comets of 1843 and 1880 must be different apparitions of the same body. This would give it a period of 37 years.

The great comet of 1843 was one of the most magnificent comets which have appeared. It suddenly made its appearance on February 28, having passed its perihelion passage on the previous day, and was so brilliant that it was distinctly visible at midday, quite close to the Sun, and could even be seen in broad daylight with the naked eye. It passed so close to the Sun, that in one day it swept over three-fourths of its entire orbit, and moved off into space with enormous velocity, carrying a great and brilliant tail as it went. This comet excited much attention at the time, and numerous attempts were made to calculate its orbit, and determine whether it had been previously seen. The resulting orbit was uncertain, especially as to the period of the comet, the observations being too close together to enable its true period to be made out. Any period from 7 to 700 years seemed possible.

It was suspected that this comet of 1843 might be identical with a very similar comet which suddenly made its appearance in March 1668, and was remarkable for its great and brilliant tail: its orbit seems to be almost identical with that of 1843. Another comet which it was also thought might be identical with that of 1843, was the comet seen in 1689; the orbits closely resembling each other. So do the orbits of brilliant comets which became suddenly visible in 1695 and 1702. The very bright comet of 1618 also seems to have had a similar orbit, and to have been of very analogous character. For this reason it was suggested that perhaps all these comets were apparitions of the same body, with a period of seven years, though it seemed extraordinary that it should not have been visible between 1702 and 1843.

If the period be taken at thirty-seven years, as indicated by the interval between 1843 and 1880, it is obvious that its prior appearances must have been in 1732, 1695, 1658, and 1621, which would exclude all these comets but that of 1695, which both Mr. Hind and Prof. Weiss think was probably an apparition of this comet. It is also thought that it might possibly be the same comet as that seen in 1618. From the position occupied by this comet it is one which may easily be overlooked at any of its reappearances, and, as pointed out by Mr. Marth, may have a period of only eighteen and a half years, as it would pass its perihelion in the intermediate returns in July, and very easily escape detection.

As there is a possibility of the comet having as short a period as seven years, it is probable that astronomers will be on the look-out for it at the epoch of its next possible appearance (January 1887) with the view of detecting it. If properly looked for it might possibly be seen before its perihelion passage.

BOTANY.

Sensitiveness in the Acacia.—Mr. T. L. Phipson has communicated to the French Academy of Sciences (*Compt. Rend.*, 24th May, 1880), some curious

results of experiments made on the leaves of the common acacia (*Robinia pseudacacia*). He finds that when the leaves of this tree are well expanded in bright sunlight, the application of from ten to twenty smart blows with the finger upon the terminal leaflet will cause all the leaflets of the leaf to fold up and go to sleep, just as they do at night. In one case the leaflets occupied five minutes in closing; in another instance four minutes and a half. The leaflets close one after the other, commencing with those nearest the terminal leaflet. Mr. Phipson observed that the leaves required from two to three hours of exposure to the sun before they were again fully expanded.

CHEMISTRY.

Action of Phosphonium Iodide on Carbon Disulphide.—It has been noticed by Hans Jahn (*Ber. Chem. Gesells.*, 1880, xiii., 127), that if phosphonium iodide and carbon disulphide be heated together in sealed glass tubes to 120° or 140° C., the latter is converted, by the hydrogen liberated by the breaking-up of the iodide, into marsh-gas and sulphuretted hydrogen, in accordance with the equation:—

$$C\,S_2 + H_8 = C\,H_4 + 2\,H_2S.$$

At the same time, however, a crystallizable body makes its appearance in the form of red needles, which is, probably, a molecular compound of the disulphide and the iodide; by treatment with water it is converted into an amorphous, white, easily-changeable compound, which, according to the result of analysis, appears to have the composition $C_5S_2\,P_6\,H_6\,O_{12}$. By the action of water on the pure phosphonium iodide in presence of carbon disulphide no such compound is found. Its formation from the two bodies may, however, be represented as taking place in the following manner:—

$$5\,C\,S_2 + 6\,P\,I_2 + 12\,H_2O = C_5\,S_7\,P_6\,H_6\,O_{12} + 12\,H\,I + 3\,H_2S.$$

When boiled with water in closed tubes, the compound decomposes into carbonic acid, sulphuretted hydrogen, phosphorous acid and amorphous phosphorus, in accordance with the equation:—

$$C_5\,H_7\,P_6\,H_6\,O_{12} + 6\,H_2\,O = 5\,CO_2 + 7\,H_2\,S + 4\,H\,P\,O_2 + 2\,P.$$

As in the above compound, the atomic ratio between phosphorus and iodine in respect to oxygen is as 1 : 2, the assumption may be held that at the temperature above referred to (120° – 140° C.) the decomposition of phosphonium iodide takes place in accordance with the equation:—

$$2\,H_4PI = P\,I_2 + H_3\,P + H_5.$$

The new Elements found in Gadolinite and Samarskite.—Since the publication of the paper on Terbium by Marignac and Delafontaine, in March 1878, announcements have appeared respecting no less than ten new earths, mosandrin, philippin, ytterbin, decipin, scandin, holmin, thulin, samarin, and two others without names. As was to be expected, so extraordinary an increase in the number of a series of bodies which are exceedingly difficult to separate and hard to characterize has not been allowed to pass without question. Delafontaine (*Compt. Rend.* 1880, xc. 221) in his investigation on the composi-

tion of gadolinite and samarskite, has always laid great stress on the atomic weights. As soon as a substance shows an atomic weight higher than that of the other members of the same group (for example, decipin and ytterbin) he regards the existence of the new element as established. When, however, its atomic weight comes between those of other members of the same series (for example, philippin compared with yttria and terbia), experiments were continued until the chemist was convinced that the body under investigation was not a mere case of a mixture of others. In such cases a study of the physical characters was of value. On these grounds the author regards as established the existence of ytterbium, decipium, and philippium. Of scandium, he can say nothing, as he has not met with it. Of mosandrium, he is firmly of opinion that it must be struck out of the list of elements. On samarium, Lecoq de Boisbaudran published a note, in August, 1879, and in the preceding February, one on another new earth in samarskite. The proportions of the latter appear to differ but slightly from those of decipin, or a mixture of that oxide with terbia; and the character of samarium, chiefly known by its absorption-bands, leaves the existence of that substance doubtful. Marignac and Soret have shown erbin to be a mixture of several bodies; and this result Cleve, with the help of Thalén, has confirmed. Cleve goes even further in giving names to two metals which he distinguishes by their spectroscopic differences. The extreme red line noticed by Soret, he refers to thulium, and the red and green lines ($\lambda = 640$ and 536) he terms holmium. Samarskite contains but little of the old erbia; the spectrum of its solution shows it but faintly, especially the green and red absorption-bands ($\lambda = 523$ and 488); the indigo line, on the other band ($\lambda = 462$), is stronger. After removing the didymium, decipium, and terbium compounds, Delafontaine submitted their formiates to fractional recrystallization: the first products gave 49-47 per cent of base; they were purified and again recrystallized, whereby a product rich in terbium was obtained, and in the mother liquors another rich in yttrium. The residue, rendered incandescent, and then converted into nitrates, showed the indigo band 452 strongly; the others weaker. This nitrate, whether dissolved or in crystals, was completely colourless, and had an equivalent $= 98$. By fractional decomposition, and by ignition, it was separated into six products (A to F): A had an equivalent $= 102$, the formiate had a light rose colour, and the absorptive spectrum showed the line of impure erbia very strongly. B to E was more strongly coloured, the nitrate colourless, the spectrum showed only the band in the red 640, in the green 536, was small; and in the indigo, 448–445, broader; the line of pure erbia was reduced to a minimum. The earths, in B to E, cannot be separated into terbia and yttria in the usual way. The product F, the last we have to consider, gives a very weak spectrum; and its colour, as well as its equivalent, was less than that of the substance in the original material. The author has given the name philippin to the yellow oxide differing from terbia, which forms the chief constituent of the oxide above referred to. Its properties accord with those of the X earth of Soret, and the holmin of Cleve, so that the latter name should fall into disuse. Philippin may be regarded as consisting of two oxides, one giving the indigo line 448–445, and the other the band 640 and 536; the author, however, has met with no facts which favour this view. The so-called euxenite of North Carolina contains even less of earths

with absorptive spectra than samarskite. In addition to terbia there was found a yellow earth with an equivalent of about 90, and without an absorptive spectrum, which appeared to be a mixture of terbia and yttria.

The Vapour Density of the Halogens.—The researches on this subject, carried on by Victor Meyer, in association with Carl Meyer, on the dissociation of chlorine at a red heat, have opened the field to a great number of researches, the publication of which they withhold till all the difficulties incident to the inquiry have been overcome. In the meantime, Crofts has published in the *Comptes Rendus* the results of an investigation carried out according to the method proposed by Meyer, and they agree with those of Meyer which had not yet been made known. This has led Victor Meyer to publish his results in a series of short notices in the *Rev. deut. Chem. Gesellsch.* (1880, xiii. 34). They are directed, in the first place, to the comportment of the two other halogens at a red heat, then to the conditions under which the dissociation occurs and is persistent, and finally to the cause of it. In his paper on chlorine Meyer pointed out that iodine comported itself similarly. At 600° the density of iodine vapour corresponded exactly with the formula I_2; at 800° a considerable diminution of the density was observed, and from 1027° to 1567°, through a range of temperature extending over 500°, it was fixed and unalterable corresponding to $\frac{2}{3}$ I_2. Iodine differs from chlorine in that the decrease of density, amounting to $\frac{1}{3}$, is reached at a lower temperature: chlorine only at 1200° is completely reduced to $\frac{2}{3}$ Cl_2, while iodine shows the complete change at about 1000°. Bromine showed the same change at 1570°, when the density fell to 3·78 and 3·64; the theoretical density of Br_2 is 5·52, and of $\frac{2}{3}Br_2$ 3·64. It therefore follows the same law, its density diminishing to the extent of $\frac{1}{3}$rd its normal value at a yellowish red heat. The dissociation of chlorine to molecules of the size $\frac{2}{3}$ Cl_2, which when nascent chlorine from platinum protochloride is employed occurs at a yellow red heat, does not take place at the same temperature when ready-prepared chlorine is employed in its place; iodine, on the other hand, undergoes the change in either case. The capability of Cl_2 and $\frac{2}{3}$ Cl_2 of existing at the same temperature, which the foregoing statement implies, is analogous to that of oxygen and ozone, which, as regards their dissociation, comport themselves in exactly the same manner. It is not a little curious, that while Deville and Troost noticed no change in the density of iodine vapour at 1000°, both Meyer and Crofts have observed the diminution of $\frac{1}{3}$rd its density at that temperature: the former places the iodine in a cold vessel and slowly heats it; the latter places it at once in the highly heated apparatus.

GEOLOGY AND PALÆONTOLOGY.

Pre-Cambrian Rocks in the Scottish Highlands.—According to Dr. Hicks, in a paper communicated to the Geological Society, the North Western and Central Highlands of Scotland include the following districts in which the rocks are wholly or in part pre-Cambrian:—

(1) *Glen Finnan, Loch Shiel to Caledonian Canal.*—In the former district

the rocks are gneiss, often massive. In Glen Firmilee is a series which the author regards as newer and Pebidian. At Farofern are quartz rocks which the author identifies with those beneath the limestone in Glen Laggan, near Loch Maree, and probably of Silurian age. At Bannavie is a granite which the author considers to be Pre-Cambrian.

(2) *Fort William and Glen Nevis.*—In this district chloritic schists and gneiss occur, which the author regards as Pebidian.

(3) *Ballachulish, Glen Coe, and Black Mount.*—Chloritic schists and quartzites occur here, followed near Loch Leven unconformably by Silurian rocks. On the east of the Ardsheal peninsula, there is granite, which the author believes to be Pre-Cambrian. Going eastward from Ballachulish we have slates, probably of Silurian age. In Glencoe are granite-banded felsite, gneiss, breccia, resembling as a whole the rocks of the Welsh Arvonian group. Between the Black Mount and Loch Sullich are traces of a great Pre-Cambrian axis, bringing up the gneissic series; this is traceable also towards Glen Spean and Loch Laggan to the N.E.

(4) *Tyndrum to Callander.*—South and east of the former are gneisses and silvery mica-schists. Crystalline limestones and serpentines are associated near Loch Tay, resembling those in the Pebidian series of North Wales.

Dr. Hicks states that the Silurian (and Cambrian) rocks flank the Pre-Cambrian in lines from N.E. to S.W., and overlap Ben Ledi on the south side. Thus here, as elsewhere, subsequent denudation has removed enormous masses of the more recent rocks, only here and there leaving patches of these in folds along depressions in the old Pre-Cambrian floor.

Fossil Glutton in Britain.—Mr. E. T. Newton has announced, in the *Geological Magazine* for April, and in a paper read before the Geological Society, the occurrence in the forest bed of Mundesley, Norfolk, of a portion of the lower jaw of a glutton (*Gulo luscus*), a carnivorous mammal now confined to high northern latitudes. The specimen shows the first true molar, and the hinder half of the fourth pre-molar in place, and thus furnishes all requisite characters for its identification. It is rather smaller than the same part in the recent glutton. This is the first discovery of the animal in a fossil state, except in caves.

Fossil Chelonia.—Professor Seeley has announced to the Geological Society that after a careful examination of the specimen which Von Meyer described as probably representing an Edentate mammal, allied to *Glyptodon*, under the name of *Psephophorus polygonus*, he found that, as surmised by Professor Fuchs, it was really a Chelonian allied to the Leathery Turtle (*Sphargis*). The dermal skeleton is made up of irregularly polygonal plates of various sizes, closely resembling those of *Sphargis*, except that each plate is almost twice as large as those of that form. The plates usually show a radiate ornament on the surface. On the underside of the slab are the remains of several vertebræ, apparently from the base of the neck, and these differ from the vertebræ of all known Chelonians in having strong transverse processes for the attachment of ribs. The neural arch, like the processes, is anchylosed to the centrum. The author considers that the dermal skeleton is not represented in the carapace of ordinary Chelonia, but is represented by the granulations on the surface of the carapace of the Trionychidæ. He is hence

led to indicate three primary divisions of the Chelonian order—viz. 1. *Aspidochelyidæ*, in which the bony carapace is covered with symmetrical horny scutes, including Turtles, Emydians and Tortoises; 2. *Peltochelyidæ*, in which the bony carapace has a granular surface-structure, and is covered with an undivided dermis without scutes, including only the Trionychidæ; and 3. the *Dermatochelyidæ*, in which the carapace is not developed, but is functionally represented by a bony skeleton within the skin, as in *Sphargis* and *Psephophorus*.

Bohemian Geology.—Mr. J. E. Marr, in a paper read before the Geological Society, has given the results of his investigations of the older Palæozoic rocks of Bohemia, and they are of especial interest, especially in regard to the peculiar theory of colonies put forward by the great Bohemian geologist, M. Barrande. Mr. Marr commenced with a brief notice of the Pre-Cambrian rocks, which are gneisses and schistose limestones with intrusive eclogite; over these lie unconformably green grits, ashes, breccias, hornstones (étage A of Barrande), which the author considers to represent the Harlech Group of Wales. Etage B is unconformable with this, but conformable with C, which contains the 'primordial' fauna of Barrande; D contains the colonies. E to H are Silurian, and more calcareous than those underlying them. The base of the group is unconformable with those beneath. The following are the associated igneous rocks:—Granite, Quartz-felsite, Porphyrite, Mica-trap, Diabase, Diorite, Eclogite. The author made a comparison of the various strata with English deposits. The Pre-cambrian series much resemble the Diametian and Pebidian of Wales, the latter being étage A; étage B, the Harlech; étage C, the Menevian, probably a deep-water deposit, as is indicated by the abnormal size of the eyes of its Trilobites; the lowest bed of étage D probably represents part of the Lingula Flags of Britain. D α, 1, β seems to represent the Tremadoc Shale of Britain, and, like it, contains pisolitic iron-ore. Representatives also of the Arenig and Bala beds are found. A slight unconformity marks the base or the Silurian. Three Graptolitic zones occur. The lowest, or *Diplograptus* zone, identical with the Birkhill Shales, contains thirteen species of Graptolites; the next, or *Priodon* zone (four species), resembles the Brathay Flags; the upper, or *Colonies* zone (five species), resembling the Upper Coldwell Beds of the Lake-district. Above these follow representatives of Wenlock, Ludlow, and probably of the Passage beds. The author, with the evidence of these, discussed the 'colonies' theory of M. Barrande, pointing to the non-intermixture of species, notwithstanding the irregular repetition of the zones, the non-occurrence of these colony-species in intermediate beds, and other reasons. The stratigraphy and palæontology of several of these colonies was discussed in detail, showing it to be probable that their apparent intercalation with later faunas is due to repetition by faulting.

MINERALOGY.

The Emmet County Meteorite.—This curious meteorite fell near Estherville, Emmet County, Iowa, lat. 43° 30′ N., long. 94° 50′ W., within that region of the United States which has been remarkable for falls of meteorites, three

having fallen at Rochester in Indiana, Cynthiana in Kentucky, and Warrington in Missouri, within the space of a month. The phenomena attending the fall were of the usual character, but on a grander scale. It occurred about five in the afternoon on May 10th, 1879, with the sun shining brightly. In some places the meteorite was plainly visible in its passage through the air, and looked like a ball of fire with a long train of vapour or cloud of fire behind it; and one observer saw it a hundred miles from where it fell. Its course was from north-west to south-east. The sounds produced in its course are described as being 'terrible' and 'indescribable,' at first louder than the largest artillery, followed by a rumbling noise as of a train of cars crossing a bridge. Two persons were within two or three hundred yards of the spots where the two larger masses struck the earth. There were distinctly two explosions: the first took place at a considerable height in the atmosphere, and several fragments were projected to different points over an area of four square miles, the largest going furthest to the east. Another explosion occurred just before reaching the ground, and this accounts for the small fragments found near the largest mass. The largest mass fell within two hundred feet of a dwelling-house, at a spot where there was a hole, six feet deep, filled with water. The clay at the bottom of the hole was excavated to a depth of eight feet before the meteorite was reached. Two or three days elapsed before it was reached. The second large mass penetrated blue clay to a depth of five feet, at a spot about two miles distant from the first. The third of the larger masses was found on the 23rd February of the present year at a place four miles from the first, in a dried-up slough. On digging a hole the stone was met with at a depth of five feet. The fragments thus far obtained weigh respectively 437, 170, 92½, 28. 10½, 4, and 2 pounds. The height of the meteor is calculated to have been forty miles, and its velocity from two to four miles per second, The masses are rough and knotted, like large mulberry calculi, with rounded protuberances projecting from the surface on every side. The black coating is not uniform, being most marked between the projections. These projections have sometimes a bright metallic surface, showing them to consist of nodules of iron; and they also contain lumps of an olive-green mineral, having a distinct and easy cleavage. The greater part of the stony material is of a grey colour, with the green mineral irregularly disseminated through it. The masses vary very much in density in their different parts, the average cannot be less than 4·5. When a mass is broken, one is immediately struck with the large *nodules* of metal among the grey and green stony substance; some of them will weigh 100 grammes or more. In this respect this meteorite is unique; it differs entirely from the siderolites of Pallas, Atacama, &c., or the known meteoric stones rich in iron, for in none of them has the iron this nodular character. The larger nodules of iron appear to have shrunk away from the matrix; an elongated fissure of from two to three millimetres sometimes intervenes, separating the matrix and nodules to the extent of one-half the circumference of the latter. The only mineral which could be picked out separately has an olive-green colour; it occurs in masses, from one half-inch to one inch in size, has an easy cleavage in one direction and was found to be olivine. The same mineral occurs in minute rounded concretions in other parts of the material; and minute, almost colourless crystal-

line particles in the centre are also supposed to be olivine. Troilite exists in small quantity. A quantity of the silicates was picked over, separated as far as possible from iron, and treated with hydrochloric acid. The ratio of soluble to insoluble silicate varies very much in different parts of the meteorite, varying from 16 to 60 per cent for the soluble part. The insoluble part consisted of

		Oxygen
Silicic acid	54·12	29·12
Iron protoxide	21·05	4·67
Chromium oxide	trace.	
Magnesia	24·50	9·80
Soda with traces of K and Li . . .	·09	·023
Alumina	·03	·013
	99·29	

This is evidently the bronzite commonly found in meteorites.

The green mineral is the soluble part of the meteorite; its cleavage in one direction is very perfect, its specific gravity is 3·35, it has a hardness of almost 7, and is readily and completely decomposed by hydrochloric acid. On analysis it was found to have the composition:

		Oxygen
Silicic acid.	41·50	22·13
Iron protoxide	14·21	3·12
Magnesia	44·64	17·86
	100·35	

This mineral, therefore, is olivine. Dr. Lawrence Smith, who has examined this meteorite, describes a third silicate which is opalescent and of a light greenish-yellow colour, and cleaves readily. It was a difficult matter to obtain enough of this silicate for analysis, but an examination of 100 milligrammes gave the following numbers:

Silicic acid.	49·60	26·12
Iron protoxide	15·78	3·50
Magnesia	33·01	13·21
	98·39	

This is equivalent to one atom of bronzite and one atom of olivine, which, he says, is 'A form of silicate that we might expect to find in meteorites.' The nickel iron, as has already been stated, is abundant, sometimes in large nodules of from 50 to 100 grammes. It displays the Widmanstattian figures beautifully, and possesses the following composition:

Iron	92·001
Nickel	7·100
Cobalt	0·690
Copper	Minute quantity
Phosphorus	0·112
	99·903

A careful examination for felspar and schreibersite was made, but with a negative result. (*American Journal of Science*, June, 1880. [3] XIX. 459.)

On the Artificial Formation of the Diamond.—The second paper giving in detail the method employed with success by Mr. J. B. Hannay in forming diamonds artificially, has recently been communicated to the Royal Society, and is deserving of a short notice. The amount of hydrogen taken up by sodium was determined, and it was found that as much as thirty-two times its volume could be removed from it after it had been allowed to act on paraffin spirit. The sodium was often found to contain carbon of a hard, scaly nature, and 'this was the reaction on which my work was built.' Potassium yielded less satisfactory results; lithium acted better, the carbon which it liberates by the action of paraffin spirit will often scratch glass easily. For the diamond experiments the material was heated in iron tubes, and tube after tube exploded and all was lost. It was often found on boring open a tube that the interior was harder than the exterior and converted into steel. To meet this loss of carbon some lampblack was added to the contents of the tube. Iron tubes 20″ × 2″ × ½″ bore were used, and three grammes of sodium were employed, the tubes being filled from ⅝ to ¾ full of paraffin spirit and then welded together. The tubes exploded during the heating, and recourse was had to tubes on the coil principle, and two were constructed of the toughest bar iron, made solid, and bored out afterwards. The dimensions were 20″ × 2¾″ × ½ bore; the tubes contained three grammes of sodium, ½ gramme of lampblack, and were filled two-thirds full of paraffin spirit. The heat was kept up for eight hours; both kept tight, and furnished a little scaly carbon; other tubes containing lithium burst, and it became evident that still stronger tubes would have to be used. A tube measuring 2¾″ × 20″ × ½″ bore was employed, and in it was placed some 'bone oil' (the nitrogenous distillate obtained in the manufacture of bone char), and charcoal powder, and the tube welded up solid. It was heated to a dull red-heat for fourteen hours and allowed to cool. On opening the tube there was a great out-rush of gas, and the carbon was to a certain extent dissolved and some minute portions of it were very hard. It appeared that bone oil had the power of hardening the carbon, and if it acted upon nascent carbon it might harden it so much as to produce diamond. An experiment was made in which bone oil and paraffin were mixed, so that when an alkaline metal was made to act upon it the decomposition of the hydrocarbon might yield carbon which could be crystallized by the action of the nitrogenous liquid. The proportions taken were 90 per cent of bone oil and 10 per cent of paraffin, with lithium as the metal. A very strong tube was filled and welded together and heated to a dull red heat for fourteen hours. When bored open a very high pressure was found inside the tube; the carbon was very hard but could be crushed by agate and would not scratch it. The results, however, when two liquids were used, were so much more satisfactory, that further tubes were filled, welded and heated. A tube measuring 20″ × 4″ × ½″ bore was filled with four grammes of lithium, bone oil 90 per cent and paraffin 10 per cent, and heated for fourteen hours. When opened, a large amount of gas was given off. In the end of the tube which had been in the upper part of the furnace there was a hard, smooth

mass, adhering to the sides. The tube was cut through at this point and the black mass was pulverized, when some parts were noticed to be extremely hard. On closer examination these parts were found to be mostly transparent, and on triturating them, they were obtained free from the black matter. They turned out to be crystalline carbon, exactly like diamond. From over eighty experiments made with these tubes, Mr. Hannay obtained only three results of a successful nature. The diamond fragments were burnt in a current of oxygen and found to contain 97·85 per cent of carbon. It appeared in some further experiments as if the diamond carbon contained a little nitrogen chemically combined with it.

Artificial Formation of Scorodite.—A concentrated solution of arsenic acid was heated with iron wire in a closed tube to 140°–150°C. The wire was found, by Verneuil and Bourgeois, to be covered in a few hours with a gray, apparently amorphous, material, which soon filled the whole of the fluid. This substance is a mixture of amorphous iron arseniate, and arsenious acid in small crystals. When the heating is continued, this material disappears gradually, and is changed slowly into scorodite; while fresh, apparently amorphous, material is formed, and this is continued till the solution of arsenic acid becomes too weak to continue the reaction. This goes on for about eight days. The crystals, in composition, specific gravity, density, and crystalline form, agree in every respect with the natural crystals of scorodite. (*Compt. Rend.* 1880, xc. 223.)

PHYSICS.

Silver Films in the Camera lucida form the subject of a suggestion by Mr. J. C. Douglas to the Asiatic Society of Bengal. He points out that instruments of this kind are divisible into two classes, the opaque and the transparent. To the former belong Wollaston's and Sœmmering's; to the latter the tinted glass reflector. The former are fatiguing, the latter liable to indistinctness, from double reflection at the two surfaces of the glass plate. Silver films, on the other hand, are so highly reflective that two or more successive reflections may be used. The thickness of the film may, moreover, be modified according to the ratio desired between reflected and transmitted light, and the films may be applied on curved as well as on plane surfaces They may also be found useful in constructing microscopic illuminators.

A New Action of the Magnet on Electric Currents forms the subject of a communication to the *American Journal of Mathematics*, by E. H. Hall. It was proposed to show, that if the current of electricity in a fixed conductor is attracted by a magnet, the current should be drawn to one side of the wire, and, therefore, the resistance experienced should be increased. To test this theory, a flat spiral of German-silver wire was enclosed between two thin discs of hard rubber, and the whole placed between the poles of an electro-magnet in such a position that the lines of magnetic force should pass through the spiral at right angles to the current. The wire was about ½ millim. in diameter, its resistance about 2 ohms. The magnet was worked by a battery of twenty Bunsen cells, joined four in series and five abreast. The strength of the magnetic field was probably 15,000 or 20,000

times H, the horizontal intensity of the Earth's magnetism. Making the spiral one arm of a Wheatstone's bridge, and using a low resistance Thomson's galvanometer, so adjusted as to betray a change of one-millionth in the resistance of the spiral, Mr. Hall made thirteen series of observations, each of forty readings, with the magnet excited, inactive, and reversed, successively. The results were all but negative. It was not, however, completely shown that a magnet does not tend unsuccessfully to deflect a current. To test this point, a disc or strip of metal, forming part of an electric circuit, was placed between the poles of an electro-magnet, cutting across the lines of force. The two poles of a sensitive galvanometer were then placed in connexion with different parts of the disc, until two nearly equipotential points were found. The magnet current was then turned on, and the galvanometer was observed without result. The experiment was repeated, using gold-leaf mounted on a plate of glass or a metal strip. A decided deflection of the galvanometer was thus obtained. It was permanent, and therefore not accounted for by induction. It was reversed when the magnet was reversed; but not by transferring the poles of the galvanometer on the strip. It was just what would be expected if the current were pressed, but not moved, towards one side of the conductor. In regard to direction, if we suppose the current a single stream, flowing from positive to negative poles, *i.e.*, from carbon to zinc, the phenomena indicate that two parallel currents in the same direction tend to repel each other. By the opposite supposition they attract. It is well known that two conductors bearing parallel currents in the same direction, are drawn towards each other. It is early to decide whether these facts have any bearing on the absolute direction of the current. Quantitative determinations on the subject were also commenced. 'It is, perhaps, allowable,' says the writer, 'to speak of the action of the magnet, as setting up in the strip of gold-leaf a new electromotive force, at right angles to the primary force.'

Hall's Discovery of a New Action of Magnetism on Electric Currents is stated by Professor Rowland as follows:—Whenever a substance transmitting an electric current is placed in a magnetic field, besides the ordinary electromotive force in the medium, we have another acting at right angles to the current, and to the magnetic lines of force. Whether there may not be also an electromotive force in the direction of the current, has not yet been determined with accuracy; but it has been proved, within the limits of experiment, that no electromotive force exists in the direction of the lines of magnetic force. This electromotive force in a given medium, is proportional to the strength of the current and to magnetic intensity, being reversed when either of these is reversed. It has also been found to differ in direction in iron from that in gold or silver. In gold the effects are such as would happen were the electric current to be rotated in a fixed direction with respect to the lines of magnetic force, to an amount depending only on the magnetic force, and not on the current. This fact seems to point to another very important case of rotation, namely, that of the plane of polarized light. By Maxwell's theory, light is an electrical phenomenon, and consists of waves of electrical displacement, the currents of displacement being at right angles to the direction of propagation. If the action takes place in dielectrics, the rotation of the plane of polarization of light is explained.

After working out the complete theory as to light, on the assumption that the displacement currents are rotated as well as the conducted, he finds the result very satisfactory. He thinks there are strong grounds for supposing the two phenomena to be the same.

A new form of Siren is described by Lord Rayleigh in the *Philosophical Magazine*. Some years ago he observed that a light pivoted blade is set in rapid rotation when exposed to wind. The phenomenon is of the same character as the rotation of a slip of paper falling freely in air, which was discussed in 1854 by Professor Maxwell. In both cases, the rotation may occur in either direction, proving that its cause is not to be looked for in any want of symmetry. The present is an application of a principle, the explanation of which he thinks has yet to be discovered. A blade is cut out of sheet brass, and provided with sharp projecting points, bearing in hollows at the end of two set screws, adjusted till the blade can turn in a small wooden frame freely, but without shake. Pieces of cardboard, or metal, are made to fit the blade pretty closely, so that when all are in one plane, the aperture is almost closed. The blade, on turning, acts as a revolving stopcock. He has made several sirens on this plan, which perform well. If the wind from the bellows is admitted symmetrically, they will revolve in either direction, and soon acquire sufficient speed to give a note of moderate pitch. The position of maximum obstruction is, for small displacements, one of stable equilibrium. If a larger displacement is made, the vibration tends of itself to increase up to a certain point, or even to pass into continuous rotation; but the precise behaviour in this respect probably depends on the details of construction.

In the same paper he also describes experiments for demonstrating—

The Acoustical Shadow of a Circular Disk.—In Poisson's experiment, a bright point is observed in the centre of the shadow of a circular disk, on which waves of light are directly incident. To obtain the acoustic analogue of this phenomenon, it is advisable to use sounds of very high pitch, which have the advantage of readily exciting sensitive flames. The best results were obtained with a squeaky toy-reed. A bird-call was blown, with a pressure of four inches of water, and placed about twenty inches from a disk of fifteen inches diameter. The observation was made at a distance of twenty-four inches on the other side of the disk, and succeeded, both with the ear and with a sensitive flame. In the former case a plate of wood, bored with a hole about ¼-in. diameter, was held against the side of the head so that the hole was opposite the ear-passage. The head was moved until the position of maximum sound was determined. To verify the fact that the position of maximum sound was really at the centre of the shadow, a hole bored through the centre of the disk was closed with a cork during observation. This was afterwards removed, and then the eye could see the source of sound through the two holes. The most suitable flame was that from a pin-hole burner, brought near to flaring-point by gas pressure of about ten inches. In a subsequent experiment, a toy-reed was substituted for the bird-call, and answered better with the flame than with the ear.

The Electromotive Forces in free jets of Water have been studied by Julius Elster, under the guidance of Professor Quincke. He claims as results:—

1. For E. M. F. to arise in a free jet, the water must be in contact with a solid body.

2. E. M. F. is only evolved where the particles of fluid undergo friction, so that only a small part of the jet contributes to its development.

3. When the velocity of the jet is altered, the E. M. F. is proportional to the *vis viva* of the particles.

4. The E. M. F. varies with the nature of the bodies in contact.

From which observations he concludes: That (α) motion of a fluid by itself does not produce E. M. F. (β) Capillary currents are conditioned solely by friction of particles; in those which do not wet the wall of the tube, by friction against this; in those which do wet it, by friction against a layer of fluid condensed against the wall. (γ) Capillary currents are identical with the friction currents appearing in the rubber of an electrical machine.

The Acceleration of Gravity for Tokio, Japan, formed the subject of a paper read before the Physical Society by Messrs. Ayrton and Perry. They proceeded by the following method:—A brass ball, 2352·2 grammes in weight, was suspended by a long steel wire 0·45 millimetre in thickness, and in the earlier experiments 978·7 centimetres in length. The wire was supported from a steel knife-edge resting on a brass plate. Both the brass ball and the bob of the seconds-pendulum of the standard clock were fitted with fine pieces of platinum wire, either of which dipped into a small cup of mercury when the pendulum to which it was attached was vertical. The mercury-cups, &c., were then joined up with a battery and resistance-coils to a quick-running Morse instrument. The whole constituted what is known as a 'break-circuit chronograph,' that is, a continuous ink-mark was made on the paper run out by clockwork, broken by a very small gap each time the wire attached to the bob of the seconds-pendulum passed through the mercury. These breaks, then, in the ink line indicated seconds; if, however, both pendulums were simultaneously in the vertical line, no break was made. Hence the absence of a break in the line at the end of any special second indicated coincidence of the two pendulums; and in this way the times of a large number of coincidences could be automatically registered.

During this set of experiments they could not measure the long fine steel wire with as much accuracy as was desired, since, although they had two or three brass scales, the makers had omitted to record on them at what temperature they were correct. However, assuming that one of them was accurate at 0° C., then a rather large number of experiments gave, as the value of g, 978·8 centimetres per second as a first rough approximation.

Subsequently they obtained from the Finance Department of Japan the loan of two very beautiful brass scales, by Deleuil of Paris, and guaranteed correct at 0° C. One was graduated in millimetres; the other consisted of a brass rod, with two pieces at its ends at right angles to the rod, and the distance between the two planes of the inner surfaces of the pieces was exactly a metre at 0° C. They had, then, the means of making a far more complete series of experiments than before; but as their trial pendulum was nearly ten times as long as the seconds-pendulum of their clock, the method of coincidences was an inconvenient one; and so they merely adopted the following:—The long pendulum alone controlled the 'break-circuit chronograph;' so that the number of breaks in the line during any time

indicated the number of vibrations of the long pendulum in that time. At the commencement of the experiment, after the pendulum had been set swinging, and the paper was running out at a fairly uniform speed, a mark was made on it by tapping sharply the armature up with the finger when a chronometer, lying beside the Morse instrument, indicated a certain time; and after an hour or so, the paper being kept running all the time, a second mark was sharply made on the paper when the chronometer indicated a certain other noted time. So much paper had then run out in the interval of time shown by the chronometer; and the breaks in the line, counted carefully afterwards by two independent students, gave the whole number of vibrations of the pendulum in that time. The fraction of a vibration could also, of course, be ascertained by comparing with the length of the first line made after the first break had been produced, on tapping the armature, and repeating the same process at the end of the paper. The experiment is independent of the rate at which the paper runs out, provided, of course, it is never allowed to run so slowly that there is any difficulty in distinguishing the different breaks electrically made by the long vibrating pendulum. The mean temperature of the wire was carefully taken at each experiment.

The next point was to measure accurately the length of the wire. As it was impossible to do this satisfactorily with the wire hanging up, it was taken down without disconnecting either the knife-edge carrying it or the ball at the other end. The knife-edge was then fixed at one end of a horizontal rail, and the other end of the wire close to the ball hung over a wheel with very little friction. By this arrangement the wire in a horizontal position was, of course, stretched as much as it was in the vertical position, as far as the effect of the weight of the ball was concerned. A correction had, however, to be made for the weight of the wire itself, which, of course, caused the tension to be a little less at the bottom than at the top when the pendulum was hanging up vertically. A few centimetres of similar fine steel wire being weighed, a simple integration gave the small additional weight necessary to be added. This being done, the final result obtained was that the length of the pendulum equalled 939·09 centimetres at 0° C.; and the consequent value of g in air for Tokio, Japan, calculated from the result of about eighty thousand vibrations of the long pendulum, would be 980·06 centimetres per second per second, if the pendulum could be regarded as a simple mathematical pendulum.

Correcting Factors.—1. The two most obvious corrections to apply to this result are the corrections for infinitely small arcs, and for the air-friction —neither of which was found of any practical consequence, on account of the very small angle through which the pendulum usually swung, and that the decrement of the amplitude of the vibrations was imperceptible, even after many swings. Although, however, such a pendulum as they were using approaches very nearly a perfect simple pendulum, there are certain causes of possible error arising from its flexibility and slight elasticity which would not effect a rigid compound pendulum. To estimate the practical effect of these possible errors, it is necessary to solve generally the complete problem of a heavy ball supported by an elastic wire, one end of which is soldered to the ball and the other end to a steel knife-edge. When a sus-

pended ball is swinging in the arc of a circle, we know that near the end of a swing the attachments of the ball have to resist a tendency for the ball to turn. For since the ball has been turned in passing from its lowest to its highest position, it would continue to turn were it not stopped by the wire itself. At the end of every swing, then, there must be a slight kick; so that in fact the ball will make minor swings about its point of attachment all the time of the motion. To make this kick less perceptible, we must make the fastening of the wire to the ball capable of resisting the tendency of the ball to continue its turning motion. If we do this by soldering the wire, a smaller kick will result, and will be due to the bending-moment of the wire resisting the turning action. If there were no difficulty of construction, it might be better to get rid of this kick difficulty by making the bob capable of rotating in the plane of swinging about an axis through its centre of gravity.

2. Next, with regard to the stretching of the wire arising from variations in the centrifugal force of the ball while swinging. Since the time of a complete vibration of their pendulum was nearly 6 seconds and the arc about 30 centimetres, the velocity of the middle of its path was 15·7 centimetres per second; hence the pull on the wire, which at the end of the swing was equal to the weight or the wire, or 2352·2 grammes, was increased by less than a gramme, so that no practical extension of the wire arose from centrifugal force.

3. Shortening of the length of the wire, due to its curvature, arising from the resistance of the air making it concave in the direction of motion. It is easy to see that the shortening of the pendulum due to this cause is excessively small, and is of the same order as the lengthening arising from the centrifugal force; so that these two very small errors may be regarded as balancing one another.

Also, since it may be calculated that the period of transverse vibration of the wire is less than one-fortieth of the periodic time of the pendulum, the resistance of the air cannot tend to cause amplification of the lateral vibrations in the wire itself.

It may, therefore, be assumed that the pendulum vibrated like a rigid body, consisting of a ball of brass, a straight steel wire, and a triangular steel prism, of which the edge was the fixed axis.

The steel knife-edge had a length of about 4 centimetres, a breadth of about 1 centimetre, and a depth of $\frac{1}{2}$ a centimetre; hence its weight was about 7·8 grammes, its moment of inertia about the axis of rotation 0·98 (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 0·33 centimetre. The weight of the wire was 11·6 grammes, and its length 934·99 centimetres at 0° C. Its moment of inertia was therefore $3{\cdot}3803 + 10^6$ (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 467·49 centimetres. The weight of the brass ball was 2352·2 grammes, its moment of inertia about the axis of rotation $2{\cdot}0744 + 10^9$, and the distance of its centre of gravity 939·00 centimetres at 0° C. Of the whole system, then, the weight was 2371·6 grammes, the moment of inertia about the axis of rotation $2{\cdot}0778 + 10^9$ (gramme, centimetre), and the distance of its centre of gravity $2{\cdot}2144 + 10^6$. Consequently

$$t = 3{\cdot}0748 \text{ seconds};$$

or

$$g = 979{\cdot}58 \text{ centimetres per second per second in air,}$$

or

$g = 979{\cdot}74$ *centimetres per second per second* in vacuo *for the Imperial College of Engineering, Japan*—

a result agreeing extremely closely with the number 979·7 obtained from Clairault's formula.

In beginning this series of observations it was expected to find g to be greater than Clairault's formula gives it. Clairault's formula assumes a circular equator; Capt. Clarke has found that the equator is elliptical, one extremity of its major axis being in 15° 34′ E. longitude; and therefore Tokio is in longitude nearer a minor axis than a major one. There may be, however, a reason why g satisfies so well Clairault's formula, in spite of this excentricity of the equator. The greatest depression of the Earth's surface is only a few hundred miles to the east of Japan; and probably the diminution in g produced by this cause just counter-balances the increase of g produced by ellipticity of the equator. As for local perturbations, it is to be remarked that Tokio is situated on a very large plain, there being no hills of any magnitude within eighty miles. The geodesy of Japan is of special interest on account of the great Pacific depression, and on account of the very gradual slope of the earth's surface from Japan to China, which causes Japan to be a sort of ridge.

On the Dynamo-electric Current is the title of a paper read before the Royal Society by M. Siemens. He points out that the Gramme and Siemens machines are both subject to the drawback that an increase of external resistance causes a falling-off of the current, and that, on the other hand, short-circuiting the outer resistance, through contact between the carbons of the lamp, much increases the electric excitement of the machine, and the power necessary to maintain its motion, giving rise to rapid heating, and to destructive sparks in the machine itself. An observation in a paper of Wheatstone's is referred to, showing that a powerful current is set up in the shunt-circuit of a dynamo-electric machine, which has been since taken advantage of in the Ladd and Brush machines as a current-generator.

The chief object of this paper is to show how machines worked on the shunt system can be made to give maximum results. It seems that the resistance on the rotating helix has to be greatly reduced, by increasing the thickness of wire employed, and that on the magnets increased more than tenfold, by increasing the length and weight of coil-wire employed. The results are thus summarized,—

1. The E.M.F. increases at first rapidly with increased resistance, and then more slowly towards an asymptote.

2. The current in the outer circuit is actually greater for a unit and a-half resistance than for one unit.

3. With external resistance of one unit, about equivalent to an arc through which 30 to 40 Webers are passing, 2·44 horse-power is expended, of which 1·29 is useful, giving an efficiency of 53 per cent as compared with 45 per cent of the ordinary machine.

4. The maximum energy which can be demanded from the engine

is 2·6 horse-power, so that only a small margin is needed to suffice for the greatest demand.

5. The maximum energy which can be injuriously transformed into heat in the machine itself is 1·3 horse-power, so that there is no fear of destroying insulation in the helix by excessive heating.

6. The approximately maximum current is habitually used, so that the commutator and collecting-brushes are quite capable of transmitting it.

The new machine gives steadier light, with greater economy of power, is less liable to derangement, and may be driven without change of speed by a smaller engine, and is free from all objection when used for electro-deposition.

It enables the author to effect an important simplification of the lamp regulator, dispensing with all wheel and clockwork.

A New Zinc-Carbon Battery, patented by Mr. R. Anderson, is excited by means of hydrochloric acid, bichromate of potash, and certain other 'salts.' It may be used either with or without a porous pot. It is stated to have an E.M.F. of 2·15 Volts, to be free from local action, from internal resistance, and to be very constant.

Toughened Glass appears to be less easily penetrated by the spark of the induction coil than that of the ordinary description. Ducretet, to whom the observation is due, proposes to utilize it for the production of Leyden jars and condensers.

Rotation under the Earth's Magnetic Influence is found by Sig. Agostini to occur in a drop of mercury when an electrical current is sent vertically through it. If such a drop be placed on the pole of a steel magnet, to which one terminal of a weak battery is attached, and the other terminal introduced into the drop, it also undergoes rotation. In this manner the distribution of magnetism in bars, and the neutral points, may be studied. The motion is rendered visible by strewing lycopodium on the mercury.

A New Galvanometer has been brought before the Physical Society of Paris by M. Marcel Deprez. It consists of a series of soft iron needles suspended between the limbs of a steel horse-shoe magnet of great power. Parallel to these needles are wound a few coils of stout wire to conduct the current. It is adapted to measure currents of considerable strength.

Influence of Heat on Tuning-forks has been measured by Rudolph König. He finds that up to 50° or 60° C. it is practically constant. Thick tuning-forks are more influenced than thin ones of the same pitch, showing that change of elasticity, and not change of length in the prongs, is the efficient cause of the alteration. The influence on forks of different pitch is proportional to their vibration-numbers. Generally the period is changed $\frac{1}{8943}$ by a difference of 1° Centig. The change in pitch of the normal C_3 = 512 vibrations per second at 20° for 1° Centig. is 0·0572 vibration. König has constructed a compensated fork, which at any temperature gives exactly 512 vibrations.

Variations from Mariotte's Law have for some years occupied the attention of Mons. Amagat, who has succeeded in making exact measurements of the changes in volume of gases when submitted to the pressure of a column of mercury over one-fifth of a mile in height. He has committed the results to the *Annales de Chemie et de Physique*. The place chosen for the

experiment was the coal-mine of Verpilleux, near St. Etienne. This is 327 metres, or about 408 yards deep. The temperature at the bottom is very constant. The method of experimenting consists in introducing a tube containing pure gas, such as nitrogen, into the large cavity of the apparatus, breaking off its point below the surface of the mercury with which the cavity is filled, and screwing the vessel down. Steel tubing, in sections of 2 millim. bore, is then screwed on to any required length, more mercury being forced into the reservoir by means of a powerful force-pump, until it mounts to the top of the tube. At each increase of compression the height of the mercury-column is measured, and the volume of the compressed gas is read off by means of a cathetometer. With nitrogen, compressibility slowly increases to a maximum at 65 atmospheres, decreasing again slowly to a normal figure at 91, then decreasing rapidly until at 430 atmospheres the volume is four-fifths of that given by Marriotte's law. Having obtained a nitrogen standard, other gases could be compared more easily with this than examined individually. Tables up to 400 atmospheres were obtained for air, oxygen, hydrogen, carbonic oxide, ethylene, and marsh gas. Graphic representations were prepared, the abscissæ representing pressures in metres of mercury, the ordinates the difference between the products of the pressures into the volume and unity, *i.e.* to the variation from Mariotte's law. The most conspicuous variations occur in the case of gases near liquefaction. Hydrogen is the only gas not exhibiting a minimum of product of pressure and volume. It seems probable that other gases, if forced to assume a degree of tenuity similar to that of hydrogen by means, for instance, of elevated temperature, would yield curves more and more resembling those which it furnishes. W. H. STONE.

ZOOLOGY.

The Systematic position of the Sponges.—Dr. Conrad Keller supports the notion originally put forward by Leuckart, and further developed by Hackel, that the Sponges are truly Cœlenterate animals, and not Protozoa. At the meeting of the Swiss Society of Natural Sciences in August last, he stated that at Naples, in the spring of 1879, he had the opportunity of closely observing the development of a siliceous sponge, which he named *Chalinula fertilis*, that he ascertained the existence in this sponge of separate sexes, and that during the period of reproduction the female presents a nuptial dress, her colour varying from carmine-red to blue. The ovum undergoes complete but irregular segmentation, resulting in the formation of a larva, composed at first of two, and afterwards of three lamellæ. He traced the transformation of this larva into a young sponge, and, according to him, it gave origin to a form which, with the exception of the absence of tentacles, agreed in all essentials with a young polype. He considered that his observations, in illustration of which he exhibited drawings, prove beyond doubt that the true position of the Sponges is among the Cœlenterata, of which he would make of them a third natural division (*Spongozoa*). (*Bibl. Univ.*, December 15, 1879.)

New Classification of Crustacea.—Dr. A. S. Packard, jun., has issued a sketch of his new scheme of classification for the Crustacea. He remarks that recent researches upon the embryology of the King Crab (*Limulus*), have shown certain most unexpected resemblances to the mode of development of the Arachnida;* but these are also met with in certain Crabs and Shrimps whose development is exceptional, so that the views of some naturalists, such as E. Van Beneden and Dohrn, that the King Crab is not a true Crustacean but an Arachnid, or the next thing to it, cannot be adopted. *Limulus*, according to Dr. Packard, must be regarded as a generalized or synthetic type combining with features of its own certain resemblances to the Arachnida and the normal Crustacea. In its mode of respiration, its external gills, and its circulatory organs, it is essentially a Crustacean; but it must be separated from the normal Crustacea, and form the living representative of a sub-class equivalent to all the other living Crustacea. The fossil Merostomata (*Eurypterus, Pterygotus*, &c.), are closely allied to *Limulus*, and Dr. Packard considers the Trilobites to be nearly related to the Merostomata. For his new sub-class he proposes the name of *Palæocarida*, most of its representatives being old fossils; the normal Crustacea form his sub-class *Neocarida*.

Dr. Packard gives the following table to show the mode of arrangement of the different orders of Crustacea under these two sub-classes:—

Classification of the sub-classes and orders of Crustacea.

CRUSTACEA.	NEOCARIDA.	Decapoda.
		Stomapoda.
		Tetradecapoda.
		Phyllocarida.
		Branchiopoda.
		Entomostraca.
		Cirripedia.
	PALÆOCARIDA.	Trilobita.
		Merostomata (King Crabs, &c.)

The PALÆOCARIDA show the following characters:—Appendages of the cephalothorax in the form of legs rather than jaws; no antennæ; brain on the same plane as the cephalothoracic ganglionic ring and supplying nerves to the eyes alone; nerves to the cephalothoracic appendages sent off from an œsophageal ring; nervous system ensheathed by a ventral system of arteries; metamorphosis slight; sexes distinct.

Order I. MEROSTOMATA.—No distinct thoracic segments and appendages. (*Limulus, Eurypterus*, &c.)

Order II. TRILOBITA.—Numerous free thoracic segments and jointed appendages. (Trilobites, all extinct.)—*American Naturalist*, Dec. 1879.)

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* We may remark upon an interesting piece of evidence in favour of the relationship between the Arachnida and *Limulus*, furnished by Dr. J. Barrois' researches on the embryology of the Spiders. Dr. Barrois denominates an important stage in the development of Spiders 'the limuloid stage,' the embryo at this point having a close resemblance to the King Crabs.—See *Ann and Mag. Nat. Hist.*, March 1880.

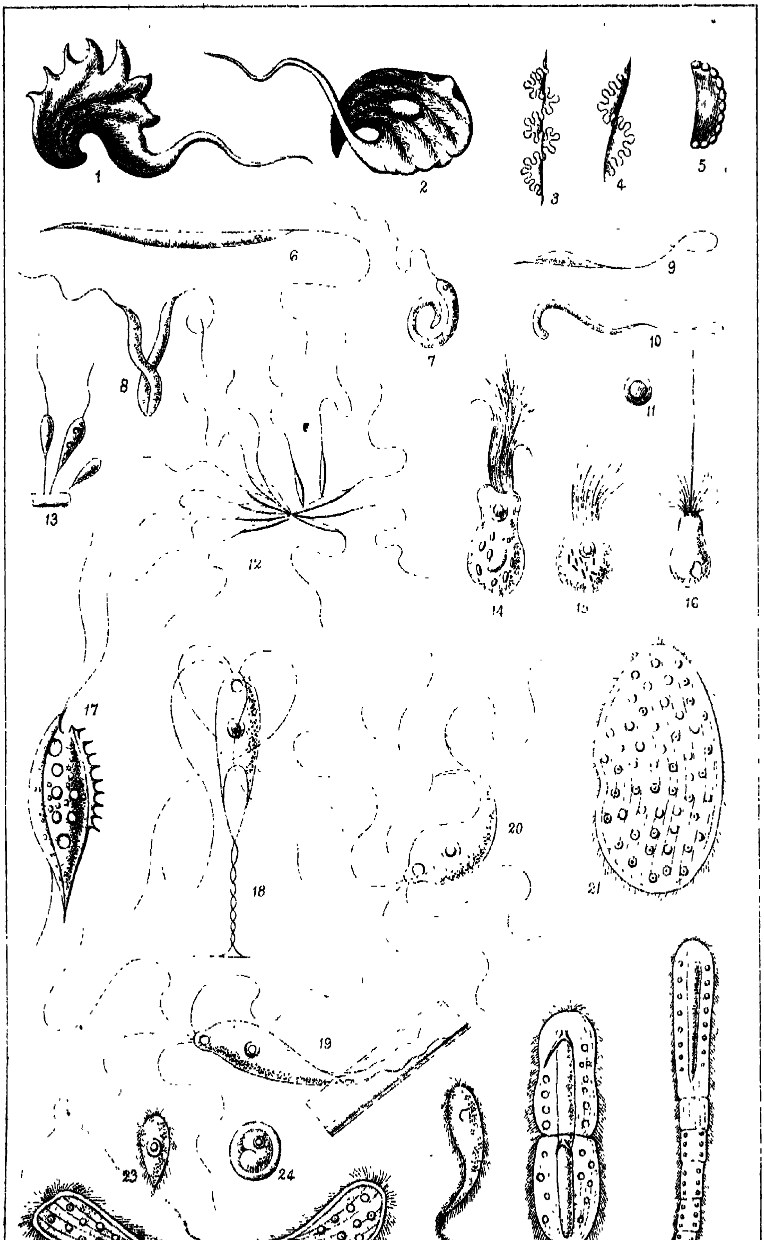
Amœba Blattæ.—Prof. Leidy brought under the notice of the Academy of Natural Sciences of Philadelphia the occurrence in the interior of the cockroach of an amœbiform parasite described by Prof. Bütschli under the name of *Amœba Blattæ.* He found it abundantly associated with two other Protozoans and two parasitic worms of the genus *Oxyuris.* Prof. Leidy thought that this amœboid parasite differed sufficiently from *Amœba* to be placed in a distinct genus, which he names *Endamœba* in allusion to its occurrence as an internal parasite; it differs from *Amœba* in the absence of a contractile vesicle, and commonly also of vacuoles, and in the want of differentiation of endosarc and ectosarc; and from *Protamœba* in the possession of a well-defined nucleus. Prof. Leidy adds:—'*Endamœba Blattæ* affords a good example of a primitive, active *nucleated organic corpuscle*, or a so-called *organic cell without a cell-wall.* In the encysted condition it would be a complete nucleated organic cell. *Endamœba* may be recommended as a convenient illustration of a primitive form of the organic cell on account of its comparatively ready access.' He gives as the size of globular forms, 0·054—0·075 millim. in diameter and of elongated forms 0·075 by 00·6 millim. to 0·15 by 0·09 millim.—(*Proc. Acad. Nat. Sci. Philad., October* 7, 1879.)

A Freshwater Medusa.—A fortnight ago zoologists were startled by a report that a small Medusa had been found living in great abundance in a tank containing fresh water in the Botanic Gardens in the Regent's Park. The tank was that in which the *Victoria regia* is grown, and the water in it is habitually kept at a temperature of 85–90° F. The case was particularly remarkable, for no known example of a Medusoid organism living in fresh water was previously known—in fact, contact with fresh water is generally fatal to such animals—and although they may be found in the mouths of rivers, it is always in that part of the tideway where the water is quite salt that they are met with. The little Medusa belongs to the so-called naked-eyed division of the late Prof. Forbes. The general run of the specimens are about the size of half a pea, but some of them are said to attain a diameter of about half an inch. They consist of a nearly hemispherical bell, from the centre of which depends a long stomachal peduncle, hanging down some distance below the margin of the bell. The bell or umbrella is traversed by four radiating canals, which start from the point of insertion of the stomachal peduncle and run to the margin of the bell. From these canals the generative organs depend, forming oval sacs between the two membranes forming the bell. These sacs contain either ova or spermatozoids, so that the animals are unisexual. The aperture of the bell, as in all naked-eyed Medusæ, is narrowed by a membrane (velum), extending inwards from the margin of the bell, which bears a great number of tentacles, four of which are considerably larger than the rest, and correspond to the radial canals. The other tentacles are much more numerous. In large specimens, according to Prof. Lankester, there are seven secondary tentacles in each of the spaces between two primary tentacles, while the interspaces between the secondary tentacles are occupied by groups of six tertiary tentacles. This gives 224 as the total number of tentacles on a fully developed specimen. The otoliths are placed along the line of attachment of the velum, and are about 80 in number; the otocysts, according to Prof. Lankester, are produced

in the form of canals into the velum, a peculiarity which he thinks will necessitate the formation of a new family or sub-order for this animal.

The little freshwater Medusa from the Victoria tank, has been described by Prof. Allman and Prof. Lankester, and has the questionable privilege of already possessing three names. Prof. Allman named it *Limnocodium Victoria*, in a paper read before the Linnean Society; Prof. Lankester in *Nature* gave it the denomination of *Craspedacusta Sowerbii*, but changed the generic name to *Periplanella*, in a communication read to the Royal Society. Nothing is known as to the means of its introduction into the tank in which it was found: this has been used for years for the cultivation of the Victoria Water-Lily, which is an annual grown from seed; the seed used is ripened in this country; and the tank is empty and dry for several months every year. Of course nothing is known of its life-history, except that it was observed by Mr. Sowerby feeding on *Daphniæ*. Prof. Lankester regards it as belonging to Häckel's order Trachomedusæ, and family Petasidæ, and as probably most nearly allied to the genus *Aglauropsis* of Fritz Müller, from the coast of Brazil. It is therefore probably one of those forms of Medusæ which have no fixed polyp-like stage in their development.

Prosopistoma.—M. Vayssière has continued his studies upon this curious insect (see *Pop. Sc. Rev.*, N. S., vol. ii. p. 444) which was supposed to be a permanent aquatic form of the Ephemeridæ. He has now observed its metamorphosis, and describes the results as follows:—Towards the end of May, the amber-yellow colour of some of the specimens which he was keeping in captivity became less bright, and he could soon see through the skin the first lineaments of the new individual. A few days afterwards (on the 3rd of June) the animals escaped from the pupal envelope in the same way as the ordinary Ephemeridæ. In the perfect state *Prosopistoma* closely resembles the well-known genus *Cœnis*, so that its right to enter into the Ephemerine family is established. Its last segment bears three rudimentary bristles, representing the natatory setæ which it possesses in its aquatic form. (*Compt. Rend.*, 7th June, 1880.)



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INFUSORIA AS PARASITES.

By W. SAVILLE KENT, F.L.S., F.R.M.S., &c.

[Plates VII. and VIII.]

An irresistible halo of fascination is ever associated with the structural details and life-phenomena of those beings to whom other and mostly higher representatives of the animal kingdom extend shelter and protection, and afford, if not direct sustenance, free lodging and a *modus vivendi*.

To the practical biologist such animal types are of peculiar interest, since such an acquired and artificial phase of existence is most often correlated with structural modifications of the most abnormal order, and which in themselves testify volumes as to the capacity and tendency of organic forms to adapt themselves to surrounding conditions, and, losing all trace of their pre-existing distinctive features, to develope into something different, or into what, for convenience sake in zoologic terminology, is denominated a new or independent 'species.' Such forms, again, recommend themselves to the evolutionist as the latest products of Nature's crucible; since, many of them being the satellites of the highest and most recently developed organic types, they must, *pari passu*, have acquired their own typical characteristics still more recently.

So far, the examples of parasitic existence with which the student of zoology is most intimately conversant, are found within the precincts of the Arthropodous and Vermiform subdivisions of the invertebrate sub-kingdom. Among the groups or types, which may be suitably cited in this connection, may be mentioned more especially the innumerable Entozoic and Ectozoic Worms, comprehended within the several orders of the Trematoda, Cestoidea, Nematoda and Acanthocephala; also the retrograde and singular Crustacean forms, represented by the ectoparasitic Lernæidæ and other Epizoa, and including also, as near relatives of the Barnacles and Acorn-shells, or Cirripedia,

those most oddly metamorphosed types, *Peltogaster* and *Sacculina*. The hosts of species that are referable to the parasitic category from among the ranks of the insect world are too innumerable, and, in many instances, too familiar, to need quotation; even the comparatively small class of the Arachnida furnishes its quota towards the augmentation of the present list, as instanced by such types as the *Pentastomum*, *Demodex*, and the abundant representatives of the Tick family, or Ixodidæ.

The object of the writer of this article is not, however, to descant at length upon the attractions presented by the members of the above-named highly-organized Metazoic groups, but to direct attention to an assemblage of organisms that exhibit parallel ecto- and endo-parasitic life-habits, and in many instances, precisely analogous, adaptive modifications with reference to their respective modes of existence, but which hitherto, on account chiefly of their exceedingly minute size, have received but scant notice. All the representatives of the group in view are, in point of fact, microscopic organisms, belonging to that subdivision of the Protozoic sub-kingdom, known as the Infusoria, for whose correct comprehension, and for a knowledge indeed of the very fact of their existence, we are dependent upon the assistance and revelations of the compound microscope.

Before entering upon a survey of the very considerable series of Infusorial forms that are associated with some one or other of the many varied phases of parasitic growth, it is desirable, perhaps, to briefly indicate the broader lines of demarcation by which this assemblage of organisms is to be distinguished from all collateral zoological groups, and also the more important secondary subdivisions, or orders, into which the Infusoria as a whole may be most naturally and conveniently subdivided. The Infusoria, as now most generally conceded, are to be regarded as unicellular animals, possessing individually the morphological value only of a simple histologic cell, having, in the majority of instances, a distinct cell-wall and enclosed nucleus, and multiplying abundantly by a simple process of binary subdivision. As here recognized, all infusorial forms possess, in addition to the foregoing essential characteristics, locomotive appendages which take the form of cilia, flagella, or prehensile, and mostly suctorial, tentacles. With but rare exceptions, the Infusoria manifest the capacity of ingesting solid food-substances, either by a distinct mouth, by many mouths, or through the general surface of the body; even yet more invariably, they possess one or more definitely located, rhythmically expanding and contracting spaces, serving as excretory organs, and to which the title of contractile vesicles is most usually applied. Various other phenomena pertaining

to the structural and reproductive phenomena of the Infusorial organism might be cited, but those already given suffice for existing exigencies.

Proceeding to an enumeration of those representatives of the Infusorial series which fall within the legitimate scope of this article, the term Parasite, as here employed, at once confronts us and demands a brief exposition of its significance. But a few years since, this term comprehended every organism that was, under any form, found associated with, or attached to, some other specific type. Credit is due, however, to the Belgian biologists, Paul and Edouard Van Beneden, for having pointed out that there is a very large assemblage of animals usually comprehended under this collective term of Parasites, that in no way live at the expense of the animal organisms, generally of higher rank, with which they are found associated, but are beholden to them only for free lodgings, providing their own larder, or, at the most, contenting themselves with the crumbs that fall from their comrades' table. For animals which maintain towards their selected host so purely amicable a relationship the title of 'Parasite,' implying an organism that lives upon or at the expense of another, has no true significance, and, recognizing the desirability of the introduction of a title that should more precisely indicate this peculiar relationship, the authors quoted have bestowed upon them the distinctive one of 'Commensals.' Familiar examples of 'Commensalism,' as distinct from 'Parasitism' in its true and restricted sense, are afforded by such forms as the well-known, so-called Parasitic Sea-anemones, *Sagartia parasitica* and *Adamsia palliata*, found attached to the shells of Gasteropodous Mollusca inhabited by Hermit crabs; and with which last-named Arthropods they are reported to cultivate so intimate an acquaintance as to secure their own transfer at the hands of their host to the next convenient abode which its constantly increasing bulk obliges it to occupy.

The extensive tribe of the Cirripedia, including the Barnacles and Acorn-shells, yield also an extensive series of Commensals; certain of them, such as the genera *Tubicinella* and *Coronula*, are respectively only found attached to, or deeply immersed within, the epidermis of various Cetacea; while another form, *Pyrgoma*, takes up its abode within the midst of the polyparies of reef-building corals, and is met with under no other conditions. These, and innumerable other species ordinarily denominated parasitic types, in no way prey upon the vital or nutrient juices of their elected host, after the manner of true Parasites, but, at the outside, simply take 'pot-luck' and a small share of the good things consumed by the latter. Quite as frequently indeed, they cater for themselves independently,

accepting only free lodging, and it may be the means of transit, at the hands of their co-sociates.

It is proposed in the present instance to direct attention only to the consideration of the innumerable forms of Infusoria which are referable to the category of 'Parasites' in the strictest and simplest sense, postponing, possibly, to a future occasion an account of those that lead a commensal life. It will be found most convenient to examine these parasitic types in association successively with the three primary classes of the Flagellata, Ciliata, and Tentaculifera, commencing with the first-named or lowest in the organic scale.

The Flagellata, but recently recognized as forming a clearly delimited and natural group of the Infusoria, are found to exhibit several very important modifications with relation more especially to their oral or inceptive systems and the character of their locomotive appendages, these furnishing indeed the basis upon which the present writer has established the various, and mostly newly introduced, orders of the class, embodied in a treatise devoted to an account of all known Infusorial organisms, now in course of publication.* The lowermost term in this series, as represented by the order of the Trypanosomata, is remarkable for containing two species only, *Trypanosoma sanguinis* and *T. Eberthi*, both notable for their essentially parasitic habits. The first of these, as represented in Pl. VII. Figs. 1 and 2, occurs abundantly in the blood of Frogs and other Amphibia; whilst the second, Pl. VII. Fig. 3–5, has been obtained as a parasite of the intestinal viscera of ducks and geese. High interest is attached to both of these, inasmuch as they represent the Flagelliferous series of the Infusoria in its most rudimentary condition. In neither instance is a flagellum distinctly developed, but in *T. sanguinis* one extremity is produced in a tag-like manner, and by the movements of this, combined with the undulations of the thinner lateral margin of its compressed body, it makes rapid progress through the fluid medium it inhabits. In *T. Eberthi* there is no tag-like anterior prolongation, the lateral margin being, however, developed as a still more conspicuous undulating, frill-like border. It is a remarkable fact that the contour of this last-named type corresponds to a very considerable extent with that exhibited by the spermatozoa of certain Amphibia, and more especially with those of *Bombinator igneus*, as originally figured by Leuckart and Siebold. This circumstance not unnaturally leaves grounds for some slight shadow of doubt as to whether this reputed species can be justly regarded as an independent form, or whether it does not represent the spermatic elements

* *A Manual of the Infusoria*, by W. Saville Kent, F.L.S., F.R.M.S. Part I. October, 1880. David Bogue, 3 St. Martin's Place.

of Amphibia which have been devoured by the water bird, and temporarily retained their vitality within its viscera. Premising, however, that the position of *Trypanosoma Eberthi* as a independent organism is established, yet another problem of interest presents itself; for until the entire life-histories of both this and *T. sanguinis* have been investigated, the possibility remains that the type found in the viscera of ducks and geese may represent an advanced phase only of the one inhabiting the blood of the Amphibia, or *vice versa*. A parallel association with two separate hosts is of common occurrence among the representatives of the Cestoidea and other endoparasitic worms.

From among the group of the typical Infusoria-Flagellata, numerous illustrations of true parasitic existence may be cited. The genus *Herpomonas*, as recently instituted by the present writer, embodies two specific forms, *H. muscæ-domesticæ* and *H. Lewisi*, distinguished, as in the case of *Trypanosoma*, by their dependence on other and more highly organized animal types for their means of existence. Both of these species correspond with each other in the possession of an attenuate and highly flexible vermicular body, which is provided at one extremity with a single long locomotive flagellum. The species first named, *Herpomonas muscæ-domesticæ* (Pl. VII. figs. 6–8), was originally referred by Burnet to the genus *Bodo*, and, as its specific name implies, occurs as a parasite within the intestinal tract of the common House-fly, not unfrequently, under such conditions, being present in such vast numbers as to almost completely fill this passage. *Herpomonas Lewisi* (Pl. VII. figs. 9 and 10) is encountered under distinct and highly remarkable conditions. For the first, and so far only extant record of this species, we are indebted to Mr. H. G. Lewis, who discovered the animals as parasites of the blood in apparently entirely healthy Indian rats in the year 1877, and figured and described them, without any name, in the *Quarterly Microscopical Journal*, as simple flagellate organisms inhabiting the vital fluids of the above-named rodents. The specific name here associated with this type has been recently conferred upon it by the present writer in honour of its discoverer. As examined by Mr. Lewis, the bodies of these animalcules were found to be highly plastic or 'metabolic,' exhibiting every variety of contour, and, excepting for this circumstance, corresponding to a considerable extent with the *Ophidomonas jenensis* of Ehrenberg. No oral apparatus, endoplast, or other differentiated structures, have so far been observed. It was ascertained that the rats infested by these flagellate parasites possessed an entirely local distribution, tenanting a restricted portion only of the premises on which they were discovered. A minute flagellate type presenting a contour closely corresponding with that of *Herpomonas*, but

having a rigid acicular body, has been recently discovered by O. Bütschli within the intestine of the Nematoid worm, *Trilobus gracilis.* A social group of this species, bearing the accompanying title of *Rhaphimonas Bütschli*, is represented in Pl. VII. fig. 12.

Among the simpler forms of the Flagellata that are notable for their parasitic mode of existence, a prominent position must be allotted to the numerous representatives of the genus *Bodo.* Hitherto made to include a very heterogeneous assemblage of flagellate types, this generic group, as recognized by the present writer, comprises only those species which, while agreeing with *Cercomonas* in the possession of two flagella, the one anteriorly and the other posteriorly inserted, differ, independently of their separate habitats, in the circumstance, that the posterior flagellum is adhesive, and utilized for the purpose of anchoring the animalcule to the surface of contiguous objects; while in *Cercomonas* this appendage is non-adhesive and simply trailing. Under the conditions above named, the endoparasitic Bodos are mostly found adhering in more or less considerable social groups to the intestinal walls of their respective hosts, but are at the same time readily detached, and accommodate themselves for awhile to a natatory existence. Familiar examples of this genus are afforded by the *Bodo ranarum* of Ehrenberg, inhabiting the intestines of the common Frog; and by *B. helicis*, *B. julidis*, and *B. melolonthæ*, of Prof. Leidy, originally referred to the genus *Cercomonas*, and inhabiting respectively the alimentary tract of an American garden-snail, centipede, and cockchafer. Two forms, *Bodo hominis* of Davaine, and *B. urinarius* of Hunter, attach themselves to the human subject; while the *B. lymnæi* of Stiebel is similarly entertained by the common pond-snail. An illustration of a typical representative of the genus *Bodo*, *B. intestinalis*, will be found at Pl. VII. fig. 13.

No apparently mouthless forms possessing two anteriorly inserted vibratile flagella only, and thus referable to the Dimastigous section of the Flagellata-Pantostomata, have as yet been recorded as leading an exclusively endoparasitic existence. That division of the same section, however, which is distinguished by the possession of more than two flagelliferous appendages, and which may thus be conveniently distinguished by the title of the Pantostomata-Polymastiga comprises several highly noteworthy forms. First among these may be mentioned the several remarkable species for which Dujardin instituted the generic title of *Trichomonas*, and which were originally described by that investigator as possessing, in addition to two or three flagella, a more or less conspicuously developed fringe of vibratile cilia. It has been recently shown by

Stein, in connexion with *Trichomonas batrachorum*, which inhabits, in common with many other infusorial types, the alimentary canals of frogs and toads, that what was previously reported to be a supplementary fringe of cilia is actually a delicate frill-like undulating membrane, corresponding closely with that possessed by *Trypanosoma Eberthi*, and assisting, in conjunction with the three or four flagella, in the locomotive function. An illustration of this species, as recently delineated by Stein, is reproduced in Pl. VII. fig. 17. Two other species of the same genus, *Trichomonas vaginalis* and *T. limacis*, are associated respectively with the human subject and the garden slug. A second endoparasitic Polymastigous form, in which the flagellate appendages are no less than six in number, four being inserted at the anterior and two at the posterior extremity of the body, is represented by the genus *Hexamita*, instituted, in common with *Trichomonas*, by Dujardin, and containing in like manner three or four well-differentiated species. The typical representative of this genus, *Hexamita intestinalis* (Pl. VII. figs. 18–20), occurs abundantly in that prolific hunting-ground for parasitic organisms, the rectum and intestine of the frog, *Rana temporaria*, and has recently, in association with examples of this Batrachian dissected at the South Kensington Biological Laboratory, been the object of investigation by the present writer. As a result of this investigation, one or two points of interest concerning the deportment of these singular animalcules in the fluid medium they inhabit were placed on record. While usually described as essentially free-swimming organisms, it has been ascertained by the writer that they possess the faculty also of attaching themselves at will to associated objects, and of passing a temporarily sedentary existence. When first transferred to the field of the microscope, no such property is exhibited, the little creatures hurrying hither and thither in the most aimless and excited manner. Gradually, however, their movements grow more tranquil, till at length scarcely an animalcule is to be seen exhibiting its natatory capacities, all with rare exceptions having attached themselves to the organic débris or other suitable fulcra, through the medium of their two trailing posterior flagella, which possess a marked adhesive function. Sometimes, the entire lengths of these filiform appendages are utilized as organs of adherence, and sometimes only their distal extremities. Under these last-named conditions, a highly remarkable modification of the movements of this animalcule has been observed. Where the adhesion is effected by the entire length of the flagella, the motion of the body is simply oscillatory, the four anterior flagella being deployed and agitated without apparently any definite plan of

action. When, however, adherence is accomplished through the medium only of the terminations of the flagella, the body gyrates rapidly, and with rhythmical cadence, from right to left and left to right, such action causing, as shown in Pl. VII. fig. 18, the adherent flagella to become twisted on each other, while the four anterior ones describe elegant undulations round the animalcule's body. It would seem highly probable that the form described by Prof. Leidy under the title of *Trichonympha agilis*, found within the intestine of the American White Ant, *Termes flavicans*, and likened by the discoverer to the performers in an American ballet, whose chief attire consisted of long cords suspended from their shoulders, whirled in mazy undulations around them as they danced, represented a species of *Hexamita* observed under the conditions just described. Phenomena precisely identical with those just recorded of *Hexamita intestinalis* have been found by the writer to obtain also in the non-parasitic and marsh-dwelling species, *H. inflata*.

One more Polymastigous Flagellate form has to be included in the list of endoparasitic species. This type, described by Prof. Stein under the name of *Lophomonas blattarum* (Pl. VII. figs. 14 and 15), is found within the intestine of the common cockroach (*Blatta orientalis*), and is distinguished for its possession of a plume-like tuft of long vibratile flagella, which are produced from the anterior extremity of its otherwise naked and sub-ovate monadiform body. The chief interest attaching to this infusorium is connected with the fact, that among all other Flagellata (as so far known), it presents the nearest approach to the remarkable pelagic type upon which Prof. Häckel has recently conferred the name of *Magosphæra planula*, and which in its most characteristic adult state consists of free-swimming spheroidal colony-stocks of animalcules, whose anterior borders are densely clothed with long flagella, much after the manner of *Lophomonas*, but whose aggregate mode of development is comparable to that of *Syncrypta* or *Uroglena*. A second species, or well-marked variety of *Lophomonas*, having a more elongate and spirally striate body, has been recently described by O. Bütschli under the name of *L. striata*.

No animalcule referable to the Eustomatous section of the Flagellata can as yet be included among the strictly parasitic species, the only type remaining to be noticed before arriving at the Ciliate division of the present organic series being the species somewhat imperfectly described by Dr. J. H. Salisbury under the name of *Asthmatos ciliaris*, delineated in Pl. VII. fig. 16. This infusorial form, possessing locomotive appendages of two orders, consisting of an anterior tuft of vibratile cilia, from the centre of which is produced a single

terminal flagellum, may be conveniently relegated to the order of the Cilio-Flagellata, and is notable for its association with the human subject. Unlike the various forms so far enumerated, it is not, however, an inhabitant of the intestinal viscera of its selected host, but attaches itself to the passages pertaining to the respiratory system, being, according to its discoverer, an active agent in the production of one form of the infection known as 'Hay asthma' or 'Hay-fever.' The sputa, and mucous matter discharged from the throat and nasal passages of patients suffering from this ailment, were in every instance found to contain living examples of this animalcule, while they were entirely absent in the instances where the health was not so affected. The inhalation of dilute carbolic acid, or quinine, together with the direct application of the first-named medium, was found to yield immediate relief, and after a few days' perseverance in its application, to effect a complete and permanent cure. A microscopic examination of the mucous discharge after the application of either of the above-named remedies revealed the presence of the animalcules in a dead state only, thus affording additional evidence of their intimate connection with the irritation diagnostic of this somewhat remarkable ailment. While it would seem certain that the organism now under discussion represented the active principle in the cases investigated by Dr. Salisbury, it would not seem to apply in all instances. A form of *Bacteria* has been reported to exert a similar influence, its growth and irritating properties being checked by like remedies; while with many constitutions all the distressing symptoms of hay-fever are apparently produced through the mere contact of grass-pollen with the mucous membrane of the nasal passages.

Proceeding to an enumeration of the very considerable number of types belonging to the Ciliate section of the Infusoria that are characterized by their parasitic habits, the first and by far the most important place has to be allotted to the family group distinguished by the title of the Opalinidæ, every representative of which is, without exception, endoparasitic in some member or another of the vertebrate or invertebrate divisions of the animal kingdom. This essential and invariable endoparasitic mode of existence, combined with the circumstance that all the numerous generic and specific types are distinguished by the entire absence of an oral aperture or of the capacity of in any way ingesting solid food particles, has influenced many biologists to regard the Opalinidæ as a group of organisms entirely distinct from all the four orders of the Ciliata as now recognized, and by some, even, as an independent animal group, having no affinity with the Infusoria,

but exhibiting a closer relationship to the Scolecida and other mouthless endoparasitic worms. As now more familiarly known there can, however, be but little doubt that these animalcules must be regarded as specially modified and retrograde representatives of the ordinary Holotrichous Ciliata, which, by long exposure to an endoparasitic mode of existence, have in a parallel manner lost all trace of an oral system, and developed a capacity of absorbing the nutritious and protein-laden fluids in which they are constantly immersed, through the general surface of their cuticle. As originally instituted, the group of the Opalinidæ included only the single representative genus *Opalina*; recent research has, however, so extended the number of known species that it has been found convenient to separate them into four generic groups, each distinguishable by well-marked and highly characteristic structural modifications. In the simplest integer of this series, for which alone the original generic name of *Opalina* is now retained, structural differentiation is so rudimentary or obscure that its members were for a long while reported as being entirely devoid even of those two elementary histologic elements which, with scarcely an exception, are distinctly recognizable in every other known member of the infusorial world, and represented by the endoplast, or nucleus, and rhythmically expanding and contracting spaces known as contractile vesicles. That these simple *Opalinæ* are devoid of the structure last referred to has been confirmed by the latest and most exhaustive investigations; the same researches, as conducted by Engelmann and Ernst Zeller, have, however, revealed the fact that endoplasts, or nuclei, are not only invariably represented, but in many species occur in each adult individual zooid as innumerable very minute spheroidal bodies distributed at short intervals throughout the entire substance of the cortex, the animalcules in such cases, like some few other Infusoria and the Foraminifera, being essentially multinucleate. By some, it is held that this multinucleate structure of the *Opalinæ* militates substantially against their acceptance as unicellular organisms, and indicates their closer affinity to the multicellular or metazoic animal series. Neither here, however, nor in any other of the multinucleate representatives of the sub-kingdom Protozoa, is the slightest trace exhibited of a tendency of the body protoplasm to become subdivided into subordinate or cellular areas, as is essential to the composition of the metazoic body. It is nevertheless evident, that, in all these multinucleate forms a first step is made towards an acquirement of this higher metazoic organization, and that, it is in association with such abnormal types that the transition from one to the other of these two primary zoological groups is, if anywhere, to be successfully sought. It is an

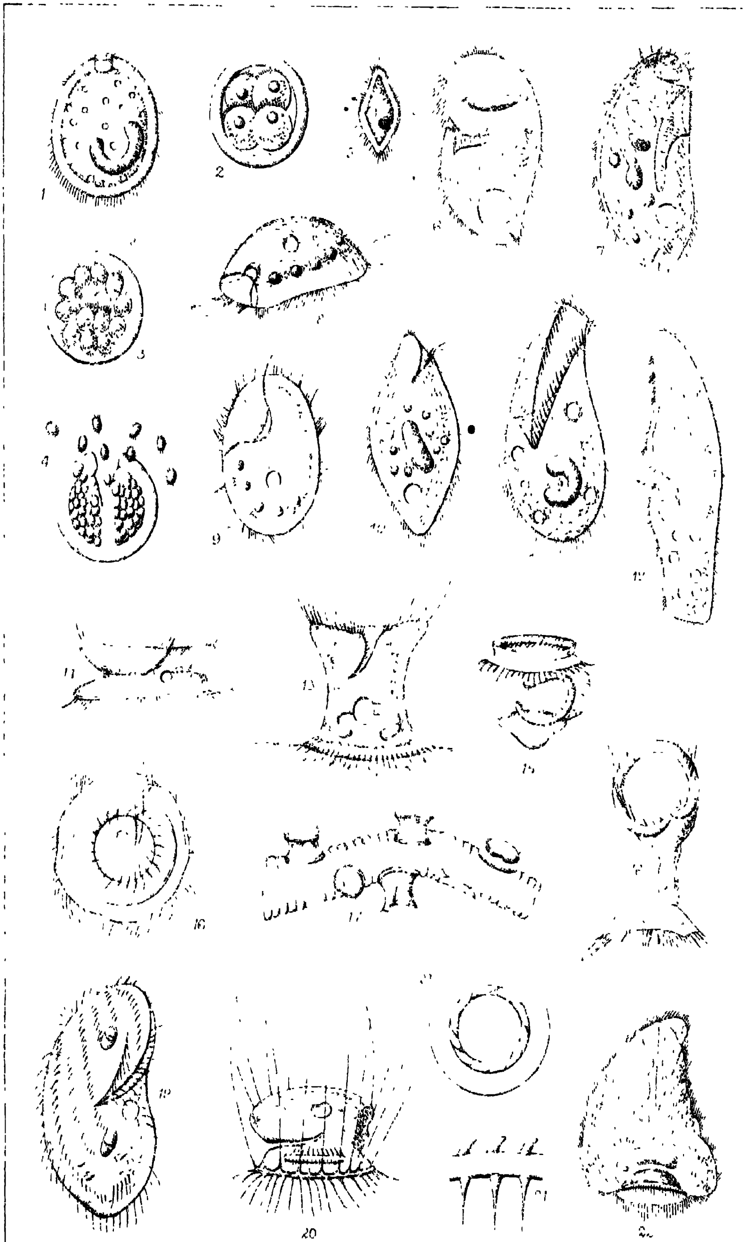
interesting circumstance in connection with the life-history of the multinucleate species of the genus *Opalina*, here referred to, that they commence existence with the possession of only a single endoplast or nucleus, and, after arriving at the full size or multinucleate condition, become subdivided by repeated fission into smaller and smaller fragments that possess finally only a single endoplastic element, each such ultimate fragment, after encystment and a more or less prolonged quiescent state, repeating the developmental cycle. The several species of *Opalina* proper occur invariably as endoparasites of the intestinal viscera of the tailed and tailless Amphibia; the most familiar type, as illustrated by *Opalina ranarum* (Pl. VII. figs. 21–25), being an almost constant guest of the common Frog (*Rana temporaria*).

The second generic group of the Opalinidæ, upon which Prof. Stein has conferred the title of *Anoplophrya*,—occurring abundantly as endoparasites of marine and fresh-water worms, molluscs, and other invertebrata,—exhibits a marked advance in complexity upon the one last described, there being in addition to a conspicuously developed endoplast, which mostly takes a band-like form, and is produced centrally through the entire length of the body, an equally distinct contractile vesicular system. Such system, moreover, is not represented, as in the majority of the Infusoria, by a single rhythmically contractile space, but by a number of such spaces, which are generally distributed in a single, or it may be a double, chain-like series, down one or both of the lateral borders. It is this monadiform modification of the disposition of the structures in question that doubtless leads to that lineal and canal-like development of this structure that is met with in certain other types, and which, indeed, by artificial pressure, may be produced in the forms now under consideration. A highly interesting circumstance, pertaining to the development phenomena of certain representatives of the genus *Anoplophrya*, is connected with the peculiar modification of the ordinary process of binary subdivision. In most instances, it happens that the animalcule so subdividing, becomes separated, either transversely or longitudinally, into two equal or sub-equal moieties. Here, however, it more often happens that a small and altogether unequal fragment becomes separated from the posterior extremity; while in occasional instances, such as *Anoplophrya prolifera* (Pl. VII. fig. 27), an endoparasite of a marine Planarian, a number of small fragmentary portions are developed simultaneously in the posterior region of the same zooid, and become separated off in consecutive order. As already recognized by various biologists, this reproductive phenomenon accords in a most remarkable manner with what obtains among the Tæniadæ and other Cestoid worms, in their so-called 'proglottid' reproductive phase. The two

remaining genera of the Opalinidæ, *Discophrya* and *Hoplitophrya*, yield yet additional structural modifications, suggestive of the affinities last cited. In the first of these, an adhesive organ, analogous to that exhibited by numbers of Cestoid and other endoparasitic worms, taking the form of a sucker or acetabulum, is developed at the anterior extremity; while in *Hoplitophrya*, a more or less complex series of horny hooklets is developed in the same region. These hooklets, as represented in *Hoplitophrya armata* (Pl. VII. fig. 26), a parasite of the Earthworm, at once recall to mind the structures having a like form and consistence common to a very considerable number of parasitic worms, and which are utilized by them in a corresponding manner for securing a firm hold upon the intestinal wall of their chosen host.

The animalcules referable to the Ciliate order of the Infusoria next to be mentioned, are distinguished for their ectoparasitic habits. Among these, the genus *Conchophthirus*, represented by three known specific types, is found associated with the mucous body-slime of various Mollusca. *Conchophthirus anodontæ* is thus, as its name implies, an ecto-parasite of the fresh-water Mussel; while *C. Steenstrupii* (Pl. VIII. figs. 8 and 9), is similarly attached to the Garden-Snail, *Helix hortensis* A yet more remarkable, and in this instance, highly instructive infusorial form, is encountered in the type described by M. Fouquet, under the title of *Ichthyophthirius multifiliis*. This species (Pl. VIII. figs. 1–5) attaches itself to young trout, excavating more or less extensive depressions in the cuticular surface, and there multiplying to such an extent as to induce a morbid condition, and, unless checked in time, fatal results. The fish-hatching establishment in France recently suffered very severely through the attacks of this microscopic parasite. A remarkable fact is connected with the reproductive phenomena of this species; it is one of the very few ciliate Infusoria that are propagated by the subdivision of its mass into a considerable number of minute spore-like elements. While rare among the Ciliate class of the Infusoria, the sporuloid mode of reproduction is of common, and almost universal, occurrence among the comparatively lowly organized Flagellata.

Arriving at the order of Heterotricha, distinguished by their possession of a conspicuous series of larger adoral cilia, in addition to the finer ones which clothe the entire surface, the three genera, *Balantidium*, *Nyctotherus*, and *Plagiotoma*, are specially notable for their parasitic habits. Three out of five of the known species of the first-named genus occur in company with the Opalinidæ as endoparasites of the frogs and . One species, however, *Balantidium coli* (Pl. VIII. fig. 10), been obtained from the colon of the human subject; while



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PARASITIC INFUSORIA.

the fifth form, *B. medusarum*, is described by Mereschkowsky as taking up its residence within the alimentary and radial canals of various small Medusæ, *Eucope* and *Bougainvillia*, obtained from the White Sea. Among the Heterotrichous genera just enumerated, the highest physiological interest is undoubtedly attached to that of *Nyctotherus*. Its several representatives inhabit, as endoparasites, the intestinal viscera of various insects and myriopods, or in the case of *N. cordiformis* of Frogs and Toads. All are remarkable for a structural peculiarity which distinguishes them in a marked manner from all other known infusorial forms, such peculiarity consisting in their possession of a distinctly prolonged anal passage, which, produced from the posterior region towards the centre of the body, leaves but a little interspace between its termination and that of the conspicuously ciliated œsophageal passage produced inwards from the opposite or anterior extremity. But a slight prolongation of these two tubes is required to effect their junction, when, a complete alimentary tract in its most rudimentary form, as possessed by the Proctuchous Turbellarians, and other lower Metazoa, would be represented. Examples of this genus, as represented by *Nyctotherus velox* and *N. Gyoeryanus*, are delineated in Pl. VIII. figs. 6 and 7. It is a remarkable fact that this most highly organized infusorial type should be found side by side with the Opalinidæ, in which the alimentary system is present in its most rudimentary form, being, in point of fact, entirely aborted. This seeming anomaly is, however, satisfactorily explained in association with the circumstance that *Nyctotherus* has developed a taste for, and partakes freely of, the solid meats; while the Opalinidæ, as babes in evolution, restrict themselves to the fluid nutriment provided side by side in their chosen habitat. The genus *Plagiotoma*, represented by a single species, *P. lumbrici* (Pl. VIII. fig. 12), corresponds considerably with *Nyctotherus*, but is devoid of a conspicuous anal passage. It is found in the alimentary tract of the common Earthworm.

The Peritrichous order of the Ciliata, considering its extensive limits, comprises comparatively few true parasitic species, though, on the other hand, it includes a very large number that must be relegated to the category of Messmates, or Commensals. All the representatives of Peritricha are distinguished by the character of the ciliary system, which is limited to a circular or spiral wreath, developed in the anterior region, and conducting to the oral aperture. Those types, referable to this order, that claim immediate attention are comprised within the respective family groups of the Urceolariidæ, and Ophryoscolecidæ. The first-named of these, in-

cluding the genera *Trichodina*, *Urceolaria*, *Cyclochæta*, and *Licnophora*, are mostly distinguished for their ectoparasitic mode of existence, being in the majority of instances attached to the cuticular surface of the various invertebrate hosts they patronize, and apparently deriving their nutriment from the mucous and waste material cast off by those latter. Perhaps, indeed, it would be more correct to refer them to the Commensal series.

In order to enable these Urceolariidæ to retain a firm hold upon their elected fulcrums of support, the posterior region of the body is so modified as to constitute an adhesive disc or acetabulum, usually strengthened by a complex horny ring, over which its possessor maintains so complete a control that it can affix or detach itself at pleasure, and lead a sedentary or free-roving existence. Among the more familiar examples of this highly characteristic group reference may be made to *Trichodina pediculus*, illustrated by Plate VIII. figs. 13–17, which often occurs in abundance upon the body and tentacles of the fresh-water polypes *Hydra viridis* and *vulgaris*. Other species of the same genus, *e. g* *T. Steinii*, *T. baltica*, and *T. scorpæna*, are found attached to the cuticular surface of respectively a fresh-water Planarian, a Baltic *Neritina*, and the Sea Bullhead, *Cottus scorpius*. In *Cyclochæta*, a singular modification of this same structural type, long, erect, setose filaments are developed in a crown-like manner round the body immediately above the acetabulum. The single known representative of this genus, *C. spongillæ* (Plate VIII. figs. 20 & 21), was recently discovered by Mr. W. H. Jackson infesting the cortical layer of the fresh-water Sponge, *Spongilla fluviatilis*, obtained from the river Cherwell at Oxford. *Licnophora*, while somewhat resembling *Trichodina* in outward form, is distinguished from it by the fact that, the anterior and posterior regions of the body are separated from one another by a stalk-like intermediate portion, while it is the right, and not the left limb of the adoral fringe of cilia, that descends into the oral aperture. Both of the two known species, *Licnophora Auerbachii* and *L. Cohnii*, are essentially marine, the former one, (Plate VIII., fig. 18), occurring on the Planarian, *Thysanozoon tuberculatum*, in the neighbourhood of Naples. A remarkable isomorphic or mimetic representative of the trichodinic structural type is afforded by the animalcule represented in Plate VIII., figs. 22 and 23, and upon which Mons. Claperede has conferred the title of *Trichodinopsis paradoxa*. This species, which occurs as a parasite within the intestinal and pulmonary cavities of various species of *Cyclostoma*, corresponds in all respects with the typical *Trichodinæ*, excepting that the entire surface of the body is clothed with fine vibratile cilia. This single circum-

stance renders it ineligible for admission into the ranks of the Peritricha proper, and indicates rather its Heterotrichous affinities. More correctly, perhaps, it may, like one or two other abnormal forms, be accepted as a connecting link between these respective orders.

The family of the Ophryoscolecidæ includes two genera, *Ophryoscolex* and *Entodinium*, which are notable for their occurrence as parasites within the first and second stomachs of various ruminants, such as sheep and oxen. Although first recognized as true Infusoria, and receiving their characteristic title, as here given, from Professor Stein, there can be but little doubt that they represent the forms originally discovered by MM. Gruby and Delafond, described by them in the '*Comptes Rendus*' for the year 1840, and there compared with Rotifera. All the several species are distinguished by the possession of an indurated carapace, frequently adorned with spinous processes; and both genera bear an anterior evertile and retractile adoral ciliary wreath, which in *Ophryoscolex* is supplemented by a second girdle of cilia, developed round the centre of the body. Unfortunately, no illustration of any representative of these very interesting genera has yet been published. According to the French authorities first quoted, these animalcules are found infesting the viscera of sheep in prodigious numbers. In five centigrammes of alimentary matter taken from the first and second stomachs (rumen and reticulum) of sheep, they found that no less than one-fifth of the total weight was composed of the bodies of these organisms. In the third and fourth stomachs (psalterium and abomasum), on the other hand, only dead and empty carapaces were met with, the softer nutrient endoplasm or parenchyma, having been dissolved out by the gastric juices. Upon the facts just recorded, MM. Gruby and Delafond argue, that the food supply of ruminants, though ostensibly of a purely vegetable nature, consists to a very considerable extent of lower animal organisms, which develope freely and with great rapidity in the first and second stomachs of their hosts, but are killed and assimilated on passing into the third and fourth stomachal compartments. The free Vorticellidan type recently described by Engelmann under the name of *Astylozoon fallax*, having two spinous processes at the posterior extremity, would seem to correspond so closely with *Ophryoscolex* and *Entodinum* in all essential structural details that its relegation to the same family group appears desirable.

But a single form out of the entire Hypotrichous order of the Ciliata (having vibratile cilia on the ventral surface only) has to be referred to the parasitic series now under consideration. This single type, the *Kerona polyporum* of C. G. Ehrenberg (Plate

VIII., fig. 19), closely resembles, in its generally flattened, bean-shaped contour, the cosmopolitan, free-swimming species *Chilodon cucullulus.* It is wanting, however, in the complex buccal armature that distinguishes that species, while the separate fringe of adoral or peristomial cilia is much more powerfully developed. The habitat of this animalcule is identical with that of *Trichodina pediculus,* it occurring as an ecto-parasite, often in vast numbers, on the body and tentacles of *Hydra vulgaris* and other fresh-water polypes. Like these Trichodinæ, it does not actually prey on the living tissues of its selected host, but contents itself with the waste material cast off from the cuticular surface, acting thus rather as a useful and efficient scavenger than as a parasite in the stricter and limited significance of that term.

EXPLANATION OF PLATES.

Plate VII.

Figs. 1 and 2. *Trypanosoma sanguinis,* Gruby. Parasite of blood of Frogs. × 600.

Figs. 3–5. *Trypanosoma Eberthi,* S. K. Parasite of intestinal viscera of ducks and geese. × 800.

Figs. 6–8. *Herpomonas muscæ-domesticæ,* Burnet, sp. Intestinal parasite of common House-fly. At Fig. 8, example dividing by longitudinal fission. × 650.

Figs. 9 and 10. *Herpomonas Lewisi,* S. K. Parasite of blood of Indian Rats. × 800. At 11, blood corpuscle of Rat similarly magnified for purpose of comparing relative size.

Fig. 12. *Rhaphimonas Bütschli,* S. K. Intestinal parasite of nematoid Worm, *Trilobus gracilis.* × 600.

Fig. 13. *Bodo intestinalis,* Ehr. Intestinal parasite of Frogs and Toads. × 300.

Figs. 14 and 15. *Lophomonas blattarum,* Stein. Intestinal parasite of Cockroach, *Blatta (Periplaneta) orientalis,* elongate and subspheroidal forms. × 500.

Fig. 16. *Asthmatos ciliaris,* Salisbury. Parasite of nasal and respiratory passages of the human subject. × 600.

Fig. 17. *Trichomonas batrachorum,* Perty. Intestinal parasite of the Frog. × 650.

Figs. 18–20. *Hexamita intestinalis,* Dujardin. Intestinal parasite of Frogs and Toads. 18 and 19, attached; 20, free-swimming conditions. × 800.

Figs. 21–25. *Opalina ranarum,* Purkinje. Intestinal parasite of Frogs and Toads. 21, multinucleate adult animalcule. × 100; 22, zooid dividing by transverse fission; 23, sporocyst with contained zooid; 24 and 25, young mononucleate condition.

FIG. 26. *Hoplitophrya armata*, Stein. Intestinal parasite of the Earthworm. × 100.

FIG. 27. *Anoplophrya prolifera*, Clap. and Lachm, sp. Intestinal parasite of marine planarian. × 100.

PLATE VIII.

FIGS. 1–5. *Icthyopthirius multifilis*, Fouquet. Ectoparasite of young Trout. 1, adult animalcule × 100; 2–5, sporular reproductive phases.

FIG. 6. *Nyctotherus velox*, Leidy. Endoparasite of myriapod, *Iulus marginatus*. × 100.

FIG. 7. *Nyctotherus Gyoeryanus*, Stein. Endoparasite of Water-beetle, *Hydrophilus piceus*. × 100.

FIGS. 8 and 9. *Conchopthirus Steenstrupii*, Stein. Parasite of Garden Snail. × 100.

FIG. 10. *Balantidium coli*, Stein. Intestinal parasite of the Human subject. × 100.

FIG. 11. *Balantidium entozoon*, Clap. and Lachm. Intestinal parasite of Frogs and Toads. × 100.

FIG. 12. *Plagiotoma lumbrici*, Duj. Endoparasite of the Earthworm, *Lumbricus terrestris*. × 100.

FIGS. 13–17. *Trichodina pediculus*, Ehr. Ectoparasite of the fresh-water Polype, *Hydra vulgaris*. 13–15, diverse contours assumable at will, × 300: 16, ventral view, showing toothed ring of acetabulum; 17, portion of tentacle of Hydra with *Trichodinæ* attached.

FIG. 18. *Licnophora Auerbachii*, Clap. Ectoparasite of planarian Worm, *Thysanozoon tuberculatum*. × 500.

FIG. 19. *Kerona polyporum*, Ehr. Ectoparasite of fresh-water Polype, *Hydra vulgaris*. × 200.

FIGS. 20 and 21. *Cyclochæta spongillæ*, Jackson. Parasite of fresh-water Sponge, *Spongilla fluviatilis*. 20, adult animalcule, × 200; 21, portion of denticulate horny ring, more highly magnified.

FIGS. 22 and 23. *Trichodinopsis paradoxa*, Clap. and Lachm. Parasite of intestinal and pulmonary cavities of fresh-water Molluscs, genus *Cyclostoma*. 22, adult animalcule, × 200; 23, horny ring of basal acetabulum.

ON THE OPINIONS OF VOLTAIRE AND LAPLACE REGARDING GEOLOGY.

By Prof. P. MARTIN DUNCAN, F.R.S. &c.

GEOLOGY, the science which investigates the successive changes that have modified the aspect of the surface of the earth, has passed through many phases during its long history. Depending much on the success of many other sciences, it has progressed or retrograded with them, and coming into opposition with religious tradition it has suffered during centuries. The history of geology has been closely connected with that of liberal intellectual culture in Europe, and the progress of the broad and very eclectic science has been synchronous with the development of civil and religious liberty. On the other hand, superstition and dogma have interfered with every branch of it, and the whole study had its age of mediæval darkness.

The science arose with the highest development of Grecian civilization, and made no small advance; and the Pythagorean doctrines, so full of truth regarding the phenomena of the former changes in the aspect of Nature, which have been handed down by Ovid, were the last and fullest expressions of the Grecian mind on geological subjects. No progress was made beyond them, and, indeed, they were considered heretical during the dark ages. From about 580 B.C. to the commencement of the sixteenth century of our era, no progress was made, and the published opinions of educated men regarding the earth, afford a melancholy instance of the subordination of the intellect. But light came at last, and where it might have been least expected by the student of general history. Fra Castero and Leonardo da Vinci expressed the more liberal opinions of Italian observers of Nature, and insisted that the fossil shells so constantly seen in the sub-Apennine strata, were once living, and that the Mediterranean Sea had once a larger area and had retired. Palissy inculcated correct views regarding fossils, seventy years afterwards; and his general belief in the former changes on the surface of the globe was that of the Greeks

modified to meet the peculiar views of ecclesiastical writers on the Deluge. Eighty more years were required to make any advance in the face of opposition, no longer from the old but from the new methods of religious thought and practice; and the end of the seventeenth century witnessed the development of more careful stratigraphical work, and of the grand theories of Leibnitz. The first half of the eighteenth century found geology a long way from the position which it occupied as a logical science at its close, when Hutton had elaborated his grand ideas of uniformity and continuity. Taking the age of Hutton as a stand-point, it is interesting to search out what was the nature of the common knowledge, regarding geology, amongst educated people before and about his time. What it was, may be well gleaned from the published works of two men of great genius, of very different kinds of minds, the one without any scientific animus, and the other devoted to exact knowledge. Both were distinguished Frenchmen, the one living before Hutton, and being the representative of the highest literary culture of an age of the greatest mental ferment; the other flourishing subsequently as the most distinguished mathematician of the Napoleonic age. They were Voltaire and Laplace.

Voltaire, detested as he was by the most powerful class in France, nevertheless attracted around him the advanced thinkers of his day; and his writings, so seldom read now-a-days, partly from prejudice and partly from the blatant impurity of many of them, indicate that he was unusually well read in all the subjects which were then attracting attention. He was an advanced representative man, at war with the priests and with the State when they endeavoured to override civil and religious liberty; his weapons were the keenest satire and the broadest humour, and they were none the less offensive because they were highly tempered in consonance with the manners and open vice of his age. A master of the art of ridicule, he often wore a mask of truth to render his shafts the more poisonous; and, indeed, it is often extremely difficult to know when he is serious or when joking. His scientific attainments were obtained, not in order that they should lead to further truths, but that they should be weapons against bad logic, the general disposition to accept every statement, made on authority, to be true, and against what he conceived to be unreasonable in Scripture. Take a chapter of his in his *Physique* as an example, which is headed, 'On the Changes which have occurred in our Globe, and on the Petrifactions which are pretended to be their Evidences:'—

'There are some errors which belong to the people, and there are others which only relate to philosophers. Perhaps the idea of so many physicists, that evidences are observable

over the whole earth of a general catastrophe, belongs to these last. They have found in the mountains of Hesse a stone which appears to be marked with the impression of a turbot, and on the Alps a petrified pike; they have therefore concluded that the sea and the rivers have flowed over the mountains. It was more natural to suspect that the fish, carried by a traveller, got stale, were cast away, and became petrified during a lapse of time. But this idea was too simple and not systematic enough. They say that they have discovered an anchor of a ship on a Swiss mountain; they do not reflect that people have often carried great bundles in their arms, and even cannon. It may be that they used this anchor to stop the bundles going down cracks in the rocks. It may be that they took this anchor from one of the little ports of the Lake of Geneva. It may be, finally, that the story is fabulous, and they prefer to assert that this was the anchor of a ship which was wrecked in Switzerland before the Deluge. The tongue of a dog-fish has some relation in shape to a stone, which has been called *Glossopètre*: this is enough to satisfy the physicians that these stones are as many tongues as the dog-fish left in the Apennines in the days of Noah.'

'The reptiles, when they are not in movement, usually assume the spiral shape; and it is not surprising that when they become petrified, the stone takes the shapeless figure of a Volute. It is again more natural that these are stones which formed themselves in spirals; the Alps and the Vosges are full of them. It has pleased naturalists to call these stones *cornes d'Ammon*. The fish called the nautilus, which has not been seen, and which was produced in the Indian Sea, is recognized in relation to them. Without too carefully examining whether this petrified fish is a nautilus or an eel, they conclude that the Indian Sea formerly inundated the mountains of Europe. Little shells have been seen in the provinces of Italy, of France, &c., which are stated to be originally from the Syrian Sea. I will not contest their origin; but may we not remember that the innumerable crowd of pilgrims and Crusaders carried its money to the Holy Land and brought back shells? Or would one rather believe that the Sea of Joppa and Sidon came to cover Burgundy and the Milanese? One might dispense well with the belief in one or the other of these hypotheses, and think, with many physicians, that these shells which are believed to have come from so far off, are fossils which the earth produced. We might, on the other hand, with more approach to reality, conjecture that there were formerly lakes in those localities where the shells are to be seen to-day. But whatever opinion or whatever error is inculcated, do these shells prove that all the universe has been capsized from top to

bottom? The mountains near Calais and Dover are of chalk; therefore formerly they were not separated by water. This may be, but it has not been proved. The rock of Gibraltar and near Tangier, is of the same kind, therefore Africa and Europe touched each other, and there was no Mediterranean Sea. The Pyrenees, the Alps, and the Apennines, have appeared to many philosophers to be the *débris* of a world which has changed its shape several times. This opinion, long maintained by the Pythagorean school and by others, affirms that all the habitable globe was once sea, and that the sea was long land.'

'This opinion has been more than once credited, by the examination of those beds of shells which are found heaped up in layers in Calabria, in Touraine, and other places, in earth situated far from the sea. Really there is a great appearance of their having been deposited during a long succession of ages. The sea, which has retired some leagues from its ancient shores, has returned, little by little, in other spots. From this almost insensible loss the right to believe in the ocean having covered the globe for a long time, is concluded. Frejus, Narbonne, and Ferrara, are no longer seaports: half of the little country of East Frisia has been submerged by the ocean; therefore, formerly, whales swam for ages over Mount Taurus and over the Alps, and the bottom of the sea was once peopled with men. This system of physical revolutions of the world has been strengthened in the minds of some philosophers by the discovery of M. de Louville. It is known that this astronomer went expressly to Marseilles, in 1714, to observe if the obliquity of the ecliptic was still the same as it had been determined by Pytheas nearly 2000 years before: he found it less by 20′. That is to say, according to his views, the ecliptic had approached the equator $\frac{1}{3}^\circ$, which proves that in 6000 years the approach would be an entire degree. This supposed, it is evident that the earth, besides its known movements, has another which makes it turn on itself from one pole to the other. It would happen that in 23,000 years the sun would be over the equator for a long period, and that in a period of about two million of years, all the climates of the world would have occurred consecutively, in the torrid zone and in the glacial zones.'

'Why, they say, be frightened about a period of two million of years? There are probably longer ones between the reciprocal position of the stars. We know already of a movement of the globe which is accomplished in more than 25,000 years, and it is that of the precession of the equinoxes. The passing away of thousands of millions of years is infinitely less in the eyes of the Eternal Architect of the universe, than are the turnings of a wheel in the twinkling of our eyes. This new period imagined by the Chevalier de Louville, and main-

tained and corrected by many astronomers, recalled the ancient researches of the Babylonians, transmitted to the Greeks by Alexander, and preserved for posterity by Ptolemy in his *Almageste*. The Babylonians pretended at the time of Alexander to have astronomical observations for 400,300 years. The attempt was made to reconcile the calculations of the Babylonians with the hypothesis of the revolutions in the millions of years; and finally, some philosophers concluded that each climate had its turn—now at the pole, now on the equator, all the seas having changed their places.'

'The marvellous, the vast, the grand mutations, these are the matters which are sometimes pleasing to the imagination of the most wise. Philosophers want the great changes in the scenes of the world, just as people do when they go to the play. From the stand-point of our existence and of its duration, our imagination soars up in the midst of millions of ages to see with pleasure Canada under the equator, and the seas of Nova Zembla upon Mount Atlas.'

This very considerable jumble of shrewd common sense, ignorance, and unreasonable opposition to inevitable conclusions, indicates, however, what were the common opinions of the day about the great succession of changes. There is some lively sarcasm at the service of the geologists who derive great theories from little facts, and it is interesting to find so great a thinker opposed to the Pythagorean doctrines. Yet the reasoning regarding the former connexion of England with France, and of Spain with Africa, is very ignorantly set aside. The common knowledge of the day was thus in advance of the critic; and it is interesting to note the ancient date of the truth of the continuity of sedimentary strata, except under the aspect of thinning out. The petrifactions and fossils were even in Voltaire's day a great trouble to the unscientific, although their true nature had been proved; but it is evident that the common knowledge of the day was in favour of the spontaneous origin of fossils and odd-shaped stones—the earth grew them.

These are ideas which are common enough, even in these days of educational progress. The mistake relating to the movement of the ecliptic is interesting, because it shows how thoroughly what may be called Astronomical Geology had taken hold of the minds of the educated classes, thanks to the great French mathematicians, who had followed our Newton. The question of the possibility of any great variation in the inclination of the polar axis to the ecliptic, has been of late greatly agitated, and the physicist declares against the possibility, whilst not a few geologists assert that the phenomena they perceive to have taken place, cannot be explained by any other method. It is interesting, then, to find these questions

regarding changes of climate so fully developing themselves. There is no doubt that Voltaire knew much which he did not care to advance in this chapter; but it is a characteristic of his style to sacrifice everything to the opportunity of an onslaught against what he considered unreasonable or dogmatic. Nevertheless, as will be noticed further on, Voltaire never grasped the possibility of any secular changes in the relative level of land and sea floor; subsidence on a grand scale does not appear to have been a notion within his imagination, although he appears to accept it as a cause of the production of some islands. In one essay he is sincerely uniformitarian in his views—the subject still being on the changes which have occurred on the globe. He says,—

'When one's own eyes have seen a mountain advance on to a plain—that is to say, an immense rock of the mountain become detached and cover some fields, an entire castle sink into the earth, a river swallowed up, and which comes forth from its abyss further on, the indubitable marks that a vast mass of water formerly inundated a country now inhabited, and a hundred vestiges of other revolutions, one is more disposed to believe in the great changes which have altered the surface of the earth, than a Parisian lady, who only knows that her house is built where there was formerly tilled ground, may be. But a Neapolitan lady, who has seen the subterranean ruins of Herculaneum, is less a slave to the prejudice which would have us believe that everything has always been as it is to-day.'

'Was there a great conflagration in the days of a Phaeton? Nothing is more likely: but it was neither the ambition of Phaeton nor the anger of Thundering Jove which caused the catastrophe. Similarly, in Lisbon in 1755, it was not the fires so frequently lit by the Inquisition which attracted Divine vengeance, which lit the subterranean fires, and which destroyed half of the town. For Mequinez, Tetuan, and considerable hordes of Arabs were more maltreated than Lisbon, and there was no Inquisition in those countries.

'The island of San Domingo, lately quite ruined, was not more displeasing to the Great Being than the island of Corsica. Everything is subject to eternal physical laws. The sulphur, the bitumen, the nitre, the iron, enclosed in the earth have thrown down a thousand cities by their mixtures and explosions, have opened and shut thousands of cracks, and we are threatened daily by accidents which depend upon the manner in which the world has been made, just as we are menaced in many countries during the winter by starving wolves and tigers. If fire, which Democritus believed was the first principle, has ruined one part of the earth, the first principle

of Thales, water, has also produced great changes. Half of America is still inundated with the ancient deposits of the Maragnon, of the Rio de la Plata, of the St. Lawrence and the Mississippi, and of all the rivers which increase in volume, thanks to the melting of the eternal snow of the highest mountains of the world, and which traverse the continent from one side to the other.'

'These accumulated deluges have produced great marshes almost everywhere. The neighbouring districts have become uninhabitable, and the soil which ought to have been fertilized by the hand of man, produces fish. The same thing has happened in China and in Egypt, and a multitude of centuries have been consumed in making canals and in draining the ground. Add to these long disasters the irruptions of the sea, the countries it has invaded or deserted, the islands it has detached from the continents, and it will be found that more than 80,000 square leagues have been wrecked from east to west, from Japan to the Atlas.'

Voltaire thus, with some accurate geographical knowledge, and with considerable point, argues against the popular notion of his day, of the persistence of all things. He then proceeds to argue in favour of, and against the Atlantis, and advances the evident former union of the Grecian Isles and Sicily with their mainlands as a suggestive proof of the subsidence of the land between the mainland and the Canaries. He states—on what authority is doubtful—that the Atlantic has but little depth between the Canaries and the mainland.

Voltaire's onslaught on Burnet is, of course, racy. He notices that 'one author, who has become more celebrated than useful by his theory of the earth, has pretended that the deluge wrecked the earth, and formed rocks and mountains, and put all into supreme confusion, and that one only sees a ruined world;' and that 'the author of another theory not less celebrated, sees nothing but arrangement, and decides that without this deluge this harmony would not have existed. Both consider the mountains to have followed the universal inundation. Burnet, in his eighth chapter, assures us that the earth before the deluge was regular, uniform, without mountains, valleys, and seas. The deluge did all that, and therefore we find *cornes d'Ammon* in the Apennines.' He then jeers at Woodward with great success, and remarks, 'The greater number of the philosophers have placed themselves without ceremony in the place of God; they think they can create an universe with a word.'

Voltaire asserts his right to examine, in consonance with the laws of probability, if this globe has ever been, or ever will be, so absolutely different from what it now is. He considers that people have only got to use their eyes. He examines the

mountains stated to be a disorderly mass of ruins of an ancient world, '*dispersé ça et là*,' like the ruins of a bombarded town, and finds them, on the contrary, arranged in definite order from one end of the universe to the other. 'It is, in fact, a chain of high and constant aqueducts, open in many spots, and giving that space to the rivers and arms of the sea which they require to irrigate the earth.' Noticing Burnet's confusion-dogma again, he remarks that 'the mountains serve as reservoirs for rain, and are the sources of rivers, and no one can fail to see in this pretended confusion the wisdom and benevolence of God.' He proceeds to state 'that every climate has its mountains, and that they are necessary for the machinery of the world. Animals could not live without them, for life cannot be without water. Water is evaporated by the sea, and is thus constantly purified; the winds carry it to the summits of the hills, where it is precipitated, and it is shown that there is a relation between the supply and the size of the rivers.' He does not believe in the Louville doctrine, and states that if it were true, the mountains would find themselves still in the same places, and that he cannot find proofs that the Alps and Caucasus have gone, even bit by bit, to Caffraria. He states that the 'bed of the ocean is a hollow, the remoter from the shore the deeper is it. There is not a rock in the open ocean except some islands. But if there were ever a time when the ocean has been on our mountains, if men and animals have ever lived in the hollow which now serves for the floor of the ocean, could they have existed there? From what mountains could they have received rivers? The world then must have been something quite different from what it is now. And how could this globe turn on itself, having one-half hollowed out and the other half high above, and surcharged with an ocean? How did this ocean manage to hold on to the mountain without flowing into the immense bed which Nature had hollowed out for it? The philosophers who make a world, only manage to make a ridiculous one. There is, then, no "systême" which can give the least probability to this idea so generally diffused, that our globe has changed its aspect, that the ocean has been long over habitable earth, and that men have lived formerly where porpoises and whales do now. Everything which vegetates, and that which is alive, does not change; every species has lasted invariably as the same. It would be very strange that a millet-seed should retain for ever its nature, and that the whole globe varied in its own.' He considers the formation of the Mediterranean and its water supply, and pooh-poohs the philosophers who stated that it has been formed by an accident, stating that it has always been in its place, and that the fundamental constitution of this universe

has never changed. Further on he attacks the people who are affected by the petrified turbot and pike already noticed, and says that they argue against sound physical reasoning; and adds, 'The love of the marvellous renders systematic philosophy childish; but Nature seems to please itself in uniformity and constancy, although our imagination revels in great changes.' This excessive uniformitarianism, a little contradictory to expressions used but a few lines back, is utilized by Voltaire in the next sentence. He states that Scripture tells us there has been a deluge, but there does not appear to be any other 'monument' of it than the memory of a terrible prodigy, which warns us in vain to be just.

In noticing the dendritic markings on such stones as agates, some kinds of marble, and flints, Voltaire states that 'it was not a tree or a house, or a man's face, which left an impression on the little stones at a time when they might have been soft or fluid, and that these are instances of an empire of whose power there is no doubt.'

Then he makes a jumble between dendrites and the true impressions of plants. He says, 'To say that the imprints of leaves of trees, which only grow in India have been seen on these dendrites, is it not to advance a matter barely proved? Does not such a fiction follow suit on the romance, imagined by some people, that the Indian sea came formerly into Germany, Gaul and Spain? The Huns and the Goths certainly came there, but the sea does not travel like men. It gravitates eternally towards the centre of the globe; it obeys the laws of Nature; and if it could have made this trip, how could it have brought the leaves of India to deposit them on the agates of Bohemia?' He insists upon this logic, and says that it enables him to nullify the opinion that the little fishes of the most distant seas have come to inhabit the quarries of Montmartre and the tops of the Alps and Pyrenees.

Voltaire believed that Nature amused herself in forming as many kinds of stones as animals: she produced them in the resemblance of parts of organisms, or twisted them in spirals, which some people have foolishly called *Cornes d'Ammon*. With regard to the knotty subject of the production of flints, he wants to know 'What stony juice made the thousand kinds of flint? How is it that in many of our countries not a single flint is to be seen, whilst others are covered with them? Why is it that in America, near the Amazon, none are to be found for 500 leagues? Sometimes enormous flints are to be found in the middle of fields, and close to them are others, not an inch in diameter, some are even only two or three lines across. Their specific gravity differs; in some it is that of iron, and in

others rather more. However heavy, opaque, or smooth, a pebble-flint may be, it is drilled with holes;' and Voltaire thinks that insects might occupy the million holes. 'It is an assemblage of homogeneous matters which produces an indestructible mass to the hammer. It is vitrifiable by long-continued heat in a furnace, and then it is observed that its constituent parts are a kind of crystal. But what force has joined these little crystals?'

'The attraction demonstrated to exist between the sun and the planets, between the earth and its satellite,—does it act between all the particles of the earth whilst it penetrates to the centre of the whole globe? Is this the first principle of the cohesion of bodies?' Voltaire notices that the Alps and other mountains contain many kinds of rocks, and addresses himself to show that a small quantity of vinegar will dissolve in the laboratory small portions of some of the rocks; therefore Hannibal did dissolve an escarpment which stopped the way; he states, in fact, that every child can make the experiment of Hannibal.

Ever wrong, from his inability to grasp the evidences of upheaval and subsidence in relation to a datum line, Voltaire argues against the value of shells found remote from the sea proving any former aspect of nature. He believed that what were found were of fresh-water kinds, and remarking on the immense number of snails that infest parts of France, demurs to accept the theory of Indian migration. But he makes a remark which will interest some amongst us: 'They discovered, or thought they did, some years ago, the bones of reindeer and of hippopotamus near Etampes; and hence it was concluded that the Nile and Lapland were formerly on the road from Paris to Orleans. But we may rather suspect that some lover of the curious formerly had these skeletons in his cabinet. A hundred parallel examples invite us to examine before believing.'

Voltaire wrote also on the shells of Touraine, a criticism of Buffon's really philosophical views of change in Nature, and indulged in some very remarkable observations on the nature of the scale of animated beings, and about corals.

Laplace, so well known to us by his great labours regarding the figure of the earth, treats the geology of his day — the early part of this century—in an astronomical manner. He antagonized some of the vulgar errors regarding the great depth of the ocean which were put forward in his day, and which lasted until a comparatively late date. His proofs were mathematical and related to the laws of the rotation of homogeneous and non-homogeneous spheres of the dimensions of the earth. It was believed that the ocean was not to be fathomed in many parts, and even the continuity of the sea, through the whole mass of the globe, from one side to the other, had its advocates.

This was the result of soundings taken with every possible difficulty and liable to every possible error, and of the extremely lively imaginations of the physical geographers of the day. But Laplace showed that the depth of the sea is a small fraction of the difference of the polar and equatorial axes of the globe, and of course his deductions did not relate in any way to measurements by soundings. It was a remarkable statement, and has been enhanced in value by the deep-sea soundings of late years. Taking the difference of the axes to be, in round numbers, 26 miles, the average depth of the great ocean is from 2000, 2400, to the extreme of 3000 fathoms, the middle number being the probable correct average. This gives a depth of nearly three miles to the ocean; and it is rather under one ninth of the standard mentioned.

According to Laplace, the oceans occupy irregularities of the surface of the earth; and mountains, table-lands, and the deep valleys, may be considered irregularities when curvature of strata, and great anticlinal and synclinal axes are to be seen amongst them. The depth of the bases of mountains, calculated from the direction of the downward curvatures of the lowest strata and the possible depth of the strata beneath the sea, gleaned from the dip of coast-line strata, appear to be enormous, and especially to those observers who are untrained. But the distinguished mathematician stated as his opinion that the irregularities of the surface of the earth and the agents which have to do with them, are at, and act at, a very slight depth below the surface. That is to say, relatively to the great bulk of the globe and its diameter, the space immediately beneath the surface, which has been affected by internal movements during the ages since the present irregularities have existed, is small. The depth of the earthquake focus, of the hypothetical volcanic reservoirs, and of the movement resulting in tangential thrusts sufficient to produce the grandest oceanic and continental geosynclinals and geanticlinals, or the minor anticlinoria and synclinoria of mountain-chains is slight, according to Laplace, writing in the second decade of this century. The great globe is quiescent within, and any energy still lingering on, is very superficial, or is antagonized by the enormous internal density of the mass. The energy is heat, and that was of course acknowledged by Laplace. The amount of knowledge of stratigraphical geology was not inconsiderable in his day, and the relations of the intrusive to the sedimentary rocks were understood; moreover, there was a belief in the primitive nature of granites and such highly crystalline rocks. But these and their great curvatures must have all appeared like filmy wavelets on the vast globe, to the great mathematician. It is doubtful, however, whether this was common knowledge,

for it is not consonant with the catastrophic ideas which prevailed so late in this century. It was certainly a foreshadowing of the opinions of the advanced school of the present day, which denies that any palpable amount of temperature has existed at the surface, conducted from below, since life has been in the sea and on the land; and which asserts that the hot spring, the volcano, and the measurable heat in mines represent so much available kinetic energy, the result of rock movement and earth contraction. The primary energy bears a very slight relation to the size of the globe and has not been exerted on a grand scale since the present density has existed. The exceeding slowness of the great crust movements appears to have been in relation to the comparatively small depth of surface affected by them. Laplace does not indicate the depth implicated beneath the surface in the successive movements which we may assume so altered the aspect of nature as to originate and terminate physical geographies of old—geological formations: but it may be now assumed that the movements must have diminished in intensity from the surface downwards, and that their sum total since the laying down of the Laurentian sediments has been vast. There is a proof of this which has not been much considered. Endeavour, in the mind's eye, to flatten out the curvatures of the strata of the Himalayas and Alps, and of the Continent to the north and south, treat the curvatures of the Scandinavian hills, the Oural and the Andes, in the same manner, and consider the vast space once occupied by the horizontal strata. Do the same in respect to mining districts, from off which mountains have been worn and washed away, and extend their folded and contorted strata, in the imagination, until they represent the breadth and length of their original deposition. There are no vast, gaping fissures, but, with the exception of some few lands, every part of the great continent is formed by sub-rock which once, before it was exposed to tangential thrust and curved, covered at least twice its present area. I know of no other subject in relation to geology, which impresses the mind so much regarding the secular contraction of the globe, since the first appearance of life. Laplace insists that the earth was primitively fluid, and that the present shape of the surface of the earth, covered more or less as it is by sea, is in accordance with the laws of equilibrium, a slight deformity only existing. He then proceeds to prove that the density of the layers of the earth increases from the surface to the centre, and that these layers are nearly regularly arranged around the centre of gravity. All this led up, in after years, to the knowledge that the density of the earth is greatest under the ocean, and least under the mountain-chains, that of the plains being intermediate; and also that the centres of figure and of gravity do not correspond.

tions. The Earth, as it passed the node, would year after year encounter no meteors until the perihelion approach of the cluster, when possibly the display may have occurred in the day-time, and been of such brief duration as entirely to elude detection.

The entry of this stream into the solar system probably dates back to a very remote antiquity—for there are several circumstances which conspire to prove that such must have been the case, and that it preceded, by many ages, the apparition of the Leonids, Andromedes, and some of the other periodical meteor-showers. The fact that it constitutes an unbroken ring leads to the inference that it must have existed from the earliest times in order to bring about so complete a dispersion of its particles, for on its first introduction, as a comet, to the Earth, it is to be assumed that it formed a condensed mass like the Leonids, and only appeared as a meteor-shower when the comet returned to perihelion. A very slight difference in the periodic times of the individual meteors following the nucleus must have eventually distributed them (by its cumulative effects) along the entire orbit. In other words, the original group must have undergone a process of lengthening out, until, at the present day, it consists of a parabolic zone of meteoric pellets, through which the Earth passes annually on August 10. Moreover, the radiant point of the shower often fails to become sharply defined. Several concentric streams of similar meteors appear to diverge from the region about η *Persei*, and their physical identity is unquestionable. They are merely the deflections or offshoots from the original system which must be greatly disturbed and contorted as the Earth annually intersects it. The full effects of these perturbations can hardly be estimated: many of the particles must be diverted into new orbits, and one of the results upon the main stream may be a constant widening out, so that the apparent duration of the shower must go on increasing. It now actively extends over at least eight nights; hence the width must exceed 10,000,000 miles. And some diminution in its intensity must occur at each return, unless there is a source of compensation for the expenditure of its materials upon the Earth. But though many millions of the atoms are annually consumed in our atmosphere, the effect of the thinning out will be very gradual in making itself appreciable, for, as compared with the vast assemblage which constitutes the main ring, the proportion which encounters the Earth is small indeed. As it is enveloped in the stream comparatively few of the meteors are actually intercepted. By far the greater number pass by untouched. If a ball is thrown up in a thick shower of rain it will only encounter a few drops. This may be taken as an illustration. The Earth, with its diameter of 8000 miles, can

only meet with a few meteors in its rapid flight through a zone exceeding 10,000,000 miles in width.

The period of the August meteors is uncertain. Their distribution appears to have been so effectual that the element cannot be determined. Some years give plentiful showers, but there have been no decided traces of regularly recurring maxima, as in the case of the Leonids. This may possibly be explained by the fact that the period is a long one, and would not become defined until after centuries of research. Comet III. 1862, which shows an exact resemblance of orbit to this system, was computed by Oppolzer to have a period of 121·5 years; and as there occurred a fine display of the August meteors in 1863, we cannot anticipate its periodical return until about 1964, if the calculations are reliable.

The August Perseids have been more frequently observed than any other system of shooting stars, from the fact that they are visible every year with more or less distinctness, and that, as an annual shower, they cannot be surpassed by any other display. The two celebrated streams of November 13 and 27, occasionally giving rise to showers of great splendour, are periodical in character, though it is extremely probable that a few of their meteors encounter the Earth at the regular return of the dates; notwithstanding that they may elude observation in consequence either of moonlight or cloudy weather, which, indeed, generally offers some impediment to success. But the August meteors recur annually with considerable intensity, and had attracted attention at a very remote epoch, though the phenomenon was not systematically studied until later times. It was reserved for Heis at Aix-la-Chapelle to more thoroughly investigate the meteors of August, for the previous observers, though they had ascertained the fact that the month was notable in this respect, had yet neglected to obtain any important data with regard to the number or directions of the meteors seen. Schmidt also, at Bonn, began assiduously to devote himself to this special line of inquiry. The particular night in August when the meteors were most plentifully distributed was found to be the 10th, though the numbers were subject to considerable variations in different years. Schmidt, from an average of several years of observations, gave the following as the horary number of falling stars for one observer. His results are compared with a similar average derived by Major Tupman and the writer from observations in 1869–71 and 1877–80 respectively:—

Date.	Falling stars in one hour. Schmidt.	Tupman.	Dennin
6 August.	6	36	13
7 "	11	37	23
8 "	15	45	26
9 "	29	—	44
(max.) 10 "	31	59	71
11 "	19	53	38
12 "	7	27	24

Schmidt's figures are very small and much below the numbers found in recent years. But the averages in the table are not thoroughly reliable, inasmuch as they are based upon only a few years' observations. A longer series might give a closer comparison, but it is seldom that the results of independent observers agree within small limits. There are differences in vision, modes of observation, and in position, which must obviously affect the numbers to no small degree; and the intermittent character of the meteor shower itself must give rise to discrepancies which cannot at first sight be accounted for. The horary number of meteors on August 10 may vary, according to Heis, from 160 (in 1839) to 24 (in 1867). During the last ten years the writer has found little variation in the intensity of the annual returns when the conditions of weather and moonlight are fully taken into account; and there is no question that some of the variations ascribed to the shower have no real existence, but are to be explained by the differences referred to above.

A fair comparison cannot be instituted between the horary numbers found by observers, unless the observations, from which the values are deduced, are made, in each case, at similar hours of the night; for shooting stars, though often plentiful after midnight, are comparatively scarce in the evening hours. This is readily explained by the fact that the principal radiant points of the showers are massed together in the eastern region of the sky where the Earth's orbital motion is directed, and it is obvious that in the evening hours, when the altitude of many of them is very low, and when others have scarcely appeared above the horizon, their operation is in a great measure restricted, so that only a feeble indication of their displays is perceptible at such a time. The case is entirely different at a later period of the night, when the constellations in which the several radiant points are situated have ascended high in the sky, and are in fact so placed that they may be seen to the greatest advantage. The August Perseids are always best observable in the morning hours, for the radiant point is very low on the horizon soon after dark, and a person who persistently watches it during the night, will find, with increasing

elevation of the radiant, a corresponding increase in the hourly number of meteors. In 1877, at Bristol, the eastern sky was persistently watched between $9^h\ 30^m$ and $14^h\ 30^m$, when 354 meteors were seen; and though the horary rate before 11^h was only 47, it rose to about 80 during the last half of the watch. Indeed the number of meteors observed at the end of the watch was more than double the number recorded at the beginning of it. Thus it is apparent that the most favourable time for such observations is in the morning hours, and though it is generally inconvenient for amateurs to extend their vigils thus far, the importance of doing so cannot be too strongly insisted on.

A typical feature of the Perseids is to be found in the streaks which frequently mark their course (fig. 1), and serve an extremely useful purpose in enabling the directions to be registered with great accuracy. The theoretical velocity of these meteors is thirty-eight miles per second, so that they belong to the swiftest class of such bodies, and, as such, would be individually re-

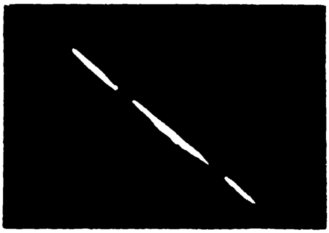

FIG. 1.—Broken Streak of a Perseid in *Pegasus*, Aug. 11, $11^h\ 10^m$.

corded with much difficulty, were it not for the special feature referred to. Their very rapid transient flights would baffle the observer as he stood endeavouring to retain the exact points of beginning and ending; and in the majority of instances he must absolutely fail to get nearer than a mere approximation. Only in cases where the meteors sped from one star to another, or in courses parallel to closely adjoining stars, could the paths be truthfully reproduced on his map. But fortunately for such investigations, we have no such difficulties to encounter. The phosphorescent line almost invariably projected on the sky by the nucleus as it rushes along, remains to guide the eye in fixing its position. It is the authentic signature of the meteor gone before, and during the brief span of its endurance the observer knows how to utilize it. It is seldom these streaks last longer than three or four seconds, though in exceptional cases of Perseid fireballs they have lingered several minutes. The writer found the average

1·8 seconds from many observations in August 1880; and the most frequent duration is about two seconds. All the brighter meteors of the shower display them. Mr. Henry Corder, of Writtle, has observed these Perseids with great diligence in recent years, and retained many interesting notes of their peculiarities. Of 910 meteors belonging to this system, which he saw in the years 1871–79, 526 were accompanied by streaks. These included 158 of the first magnitude, only 15 of which were devoid of streaks; and 243 of the second magnitude, of which 72 were streakless. Amongst the smaller members the proportion was larger. He found the brightest meteors were generally pale-green, others orange, &c.

The luminous streaks, which are known to be the ordinary characteristic of these shooting stars, have acquired a special significance from the fact that by their means the radiant point of the shower is capable of being ascertained with remarkable precision. This important element, to be reliably determined, must rest upon a large number of accurately recorded tracks, which intersect (on being prolonged backwards) at a well-defined position. Many observers have succeeded in finding this from results of more or less value. Mr. R. P. Greg analysed all the positions estimated prior to 1876, and gave the average at R.A. 44°, Dec. 56° N.; and Major Tupman, from a discussion of his own elaborate observations in the Mediterranean during the years 1869–71, derived the point $45\frac{1}{2}° + 56°$, as the centre of 28 sub-radiants. Evidently the two results, being founded on a large number of trustworthy records, and agreeing so closely as they did, showed the true radiant to be situated on the northern limit of *Perseus*, close to the star *Eta* of that constellation; and more recent determinations of a similar nature have fully corroborated that as the chief diverging centre of the August meteors. Many other contemporary showers have been detected in the same region of the heavens, but the shower of Perseids recurs year after year from its accustomed point.

During the last eleven years the writer at Bristol has awaited the annual returns of this shower, and the aggregate results of observations during the interval between the 6th and 12th August, show that 2345 meteors have been recorded, of which 1428 belonged to the display of Perseids, and 917 to other minor streams of the same epoch. In 1869 the radiant was judged to be at *η Persei;* in 1871 at *B. Camelopardi;* and in 1874 at $44° + 58\frac{1}{2}°$. The average position found during the last five years has been at $44° + 57°$; and in the diagram (fig. 2) a number of paths near this radiant are shown. Some of the meteors appear to be slightly erratic in their directions; but this may be explained either by errors of

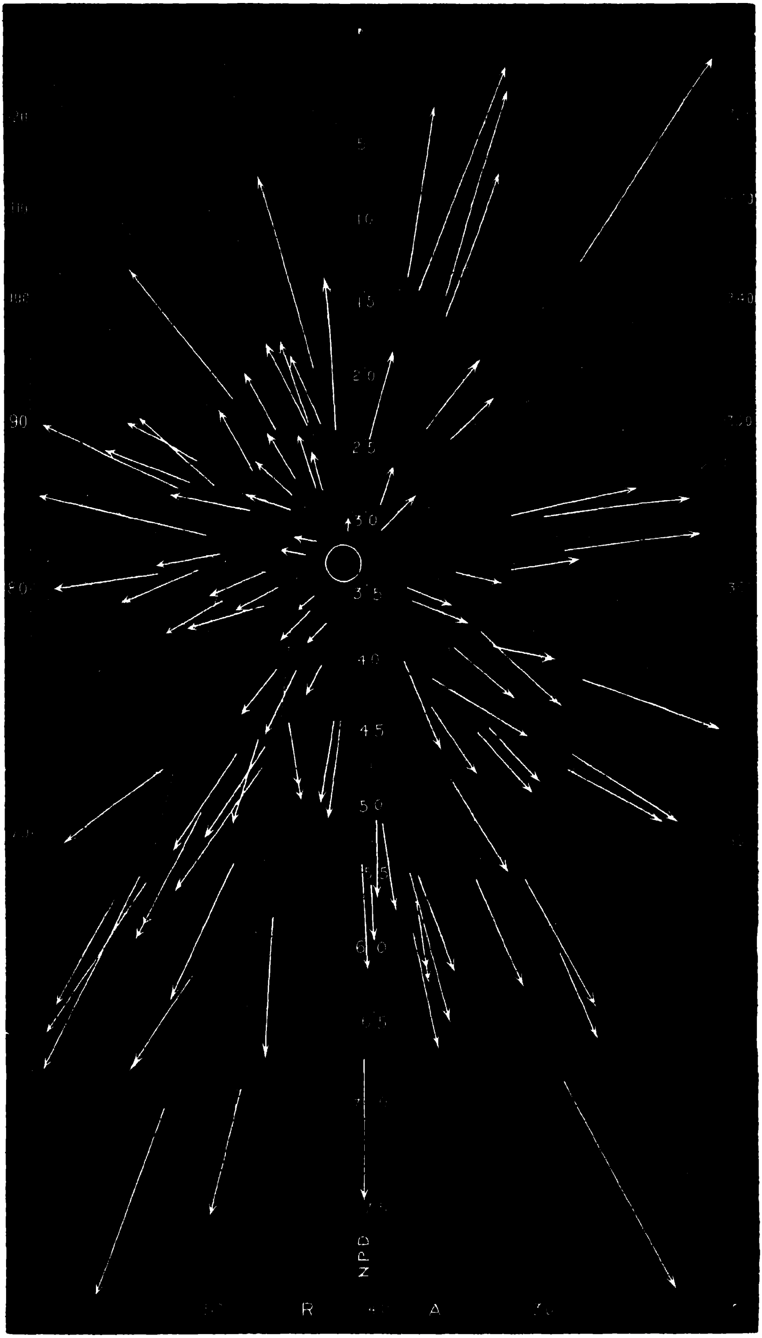

Fig. 2.—Perseids near the Radiant (44° + 57°), Aug. 8–12.

observation or by a double or diffused radiant point, which must often occasion non-conformity in the observed flights. In 1878 two points of departure were manifest from a series of precisely fixed courses at 44° + 59° and 42½° + 54°; but in 1879 the weather interrupted observations. The present year, however, afforded an exceptionally favourable opportunity to observers, and the major radiant determined by the writer was at 44° + 56°, with the decided traces of a sub-radiant at 45° + 60°. In 1878 Major Tupman found the shower double at 46° + 57°·6 and 38° + 56°; and in 1880 he strongly corroborated the results obtained at Bristol, though his observations were mainly confined to the night of August 9. At the latter station the radiant apparently advanced amongst the stars of *Perseus*, for while early in the month it was observed at 38° + 56° it had shifted to 49½° + 57½° by the 13th. The same peculiarity was noted in 1877, when the following determinations were made :—

		Radiant.			Radiant.
Aug. 3-7	...	40° + 56°	Aug. 12	...	50 + 55
Aug. 10	...	43 + 58	Aug. 16	...	60 + 59

There is a prominent display of meteors from the star-group χ *Persei* at the end of July and beginning of August, and it is possible that these showers may belong to the same system of concentric meteor streams. It is certain that this fact of a progressive radiant requires fuller elucidation, and to this end observers should keep the data obtained each night separate. It may also be suggested that the radiant point should be ascertained during each hour of observation, and then when the series are compared, any displacement must immediately become obvious, and its extent and character well defined by the observations. The meteors from *Perseus* are so numerous, and the place of divergence so readily denoted by their enduring streaks, that there will be no difficulty in an investigation of this kind. The last two years' observations have shown how exactly the radiant may be found by carefully-conducted researches, and how closely the positions derived by different observers will agree on being compared together :—

Observer.	1879, August. Chief Radiant.		1880, August. Chief Radiant.
G. L. Tupman	45° + 56°	...	44° + 56°
H. Corder	45 + 57	...	45 + 58
E. F. Sawyer	44½ + 57	...	44¾ + 56¼
W. F. Denning	46 + 58	...	44 + 56

From these values a mean of 44°·8 + 56°·8 is derived, which

is probably very near the truth. There is a secondary shower higher in declination (at about $44\frac{1}{2}° + 60°$), but this is merely a branch of the same stream, for the meteors exhibit the same specialities of appearance as those common to the major shower. An apparent diffuseness of the radiant point is often brought about by imperfectly registered tracks, and by allotting the meteors of bordering showers to the radiant of the Perseids, when in fact they belong to evidently distinct families.

A few years ago the writer undertook the investigation of these co-Perseid showers from the large mass of shooting stars which had been registered at this epoch at foreign observatories, and are contained in the published catalogues of Heis, Schiaparelli (1872), Weiss, and Konkoly. These include many thousands of paths observed during the period from Aug. 6th to 12th; and such of these as were obviously directed from radiant points situated eastwards of *Perseus*, were projected on the star-maps prepared by Prof. Herschel for the purposes of the Luminous Meteor Committee of the British Association. In all 762 meteors were thus utilized, and they gave distinct evidence of the positions of a number of active streams in *Auriga* and *Camelopardus*, some of which were previously observed by Heis, and many of them have been confirmed by the writer during the last five years. The following list embraces the chief radiants thus deduced:—

Meteor Showers east of Perseus, August 6–12.

No.	Radiant. α δ	No. of Meteors.	No.	Radiant. α δ	No. of Meteors
1	70° + 64°	74	10	134° + 77°	30
2	61 + 39	59	11	74 + 33	28
3	96 + 71	87	12	104 + 34	13
4	61 + 48	59	13	99 + 46	17
5	51 + 74	62	14	45 + 33	18
6	78 + 56	59	15	76 + 74	20
7	76 + 45	43	16	52 + 20	14
8	50 + 47	42	17	87 + 34	14
9	92 + 57	42	18	87 + 15	8

The relative positions of these showers are depicted in the diagram (fig. 3) where the more prominent displays of the group are represented by deeper circles than the minor. Some of the latter cannot yet be regarded as certainly established, inasmuch as they rest on slender materials.

Heis devoted much attention to the meteors of the August period during more than forty years (1833–75), and in his extensive 'results,' published in 1877, gives the following as the chief radiant points for August 9–11:—

Symbol.		Radiant. α	δ		No. of Meteors.	Symbol.		Radiant. α	δ		No. of Meteors
A_{11}	.	45°	+ 52°	...	233	Cr_{11}	.	273°	+ 56°	...	93
B_4	.	330	+ 55	...	164	St_{12}	.	40	+ 45	...	118
B_5	.	292	+ 70	...	135	St_{13}	.	56	+ 70	...	105
Cr_5	.	12	+ 32	...	93	St_{17}	.	27	+ 21	...	70
Cr_6	.	355	+ 81	...	103	St_{18}	.	25	+ 58	...	282
Cr_8	.	11	+ 60	...	192	St_{19}	.	295	+ 44	...	110
Cr_9	.	73	+ 63	...	125	St_{20}	.	51	+ 75	...	133

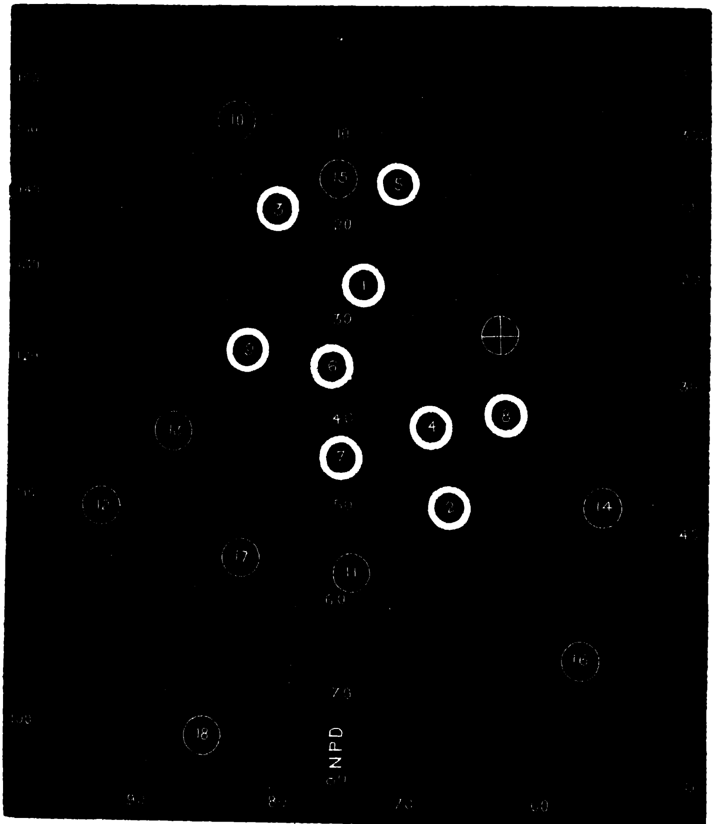

FIG. 3.—Radiant Points east of the Perseids, Aug. 6–12.

Major Showers. Minor Showers Perseids.

But in addition to these, there are a large number of radiants scattered over the sky, especially in the eastern

quadrant. One of the most notable of these proceeds from the eastern extremity of *Aries* (44° + 25°), and supplies some bright meteors in the morning hours; but the most conspicuous shower discovered east of *Perseus* at this epoch lies in *Camelopardus*, and in the diagram (fig. 4), a number of its meteors falling amongst the stars of *Ursa Major*, are reproduced from the catalogues of foreign observers. This shower, however, escaped the detection of Heis and others, who had been engaged in similar investigations, though it appears to be of more importance than several radiants in its vicinity which have been independently determined by several observers. At the end of July 1878, the writer noted a few

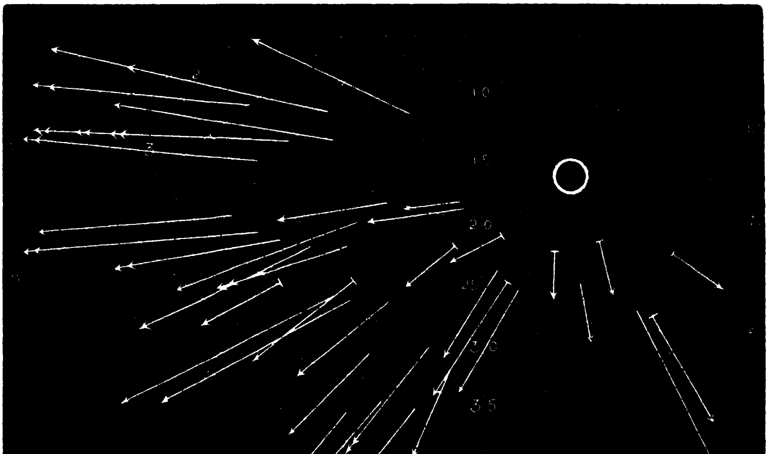

Fig. 4.—Shower from *Camelopardus* (96° + 71°), Aug. 6–12.

brilliant, slow meteors, from a point at 96° + 72°, and this may have been an early evidence of the radiant which is placed in a region bare of large stars between *Telescopium* and *Polaris*. It is just north of the triangle of faint stars (*l. p. q. Camelopardi* of Bode), east of a line drawn from *β Aurigæ* to *Polaris*, and will, no doubt, be frequently re-observed in future years, though the shower of Perseids usually monopolizes attention at the epoch of its annual returns.

There is a shower near *η Persei* (No. 2), well defined, on Aug. 6–12, Aug. 21–23, and Sept. 6–15. At the latter epoch it furnishes some fine meteors and constitutes a prominent dis-

play. The diagram (fig. 5) gives the positions of eighty-six paths conforming to this radiant, observed at Bristol, and at several foreign stations in September.

The ordinary designation of Perseids for the special meteor shower of August 10, is always understood in its individual application, though it must not be supposed that this is the only shower of Perseids visible in that month. The fact is, there are many separate showers directed from that constellation early and late in August, so that we require some distinguishing titles or symbols to conveniently particularize either of them which it may be necessary to refer to. The method now adopted of naming the chief periodical showers by the constellations in which their radiant points are situated, is very appropriate; and such displays as the Orionids, Leonids, and Geminids, have become so well known by their titles, that it would be unwise and inconsistent to attempt reform. But with regard to the minor systems, which are becoming very numerous, and require an equally ready mode of expression, there is a great difficulty in avoiding complications.

There are certainly five nearly simultaneous showers of Perseids early in August; and in every month of the year, except May and June, meteors continue to fall from that constellation. If the present mode is adopted of styling them Perseids I, Perseids II, and so on progressively, a good deal of confusion must eventually arise as new systems are discovered; and this classification by Roman numbers, however appropriate it may be in some of its other applications, will have to give way to a more distinguishing means of reference. The name at present only gives indication of the constellation from which the meteors emanate, without regard to the date or approximate place of the radiant, and it seems to me that the difficulty may be obviated by including the nearest fixed star and the epoch with that name. To render the proposal clear let us take the different streams proceeding from the undermentioned points in *Perseus* in August:—$44° + 56°$, $32° + 53°$, $61° + 36°$, $61° + 48°$, $46° + 47°$, which may be thus termed:—

η Perseids (Aug. 10).
χ Perseids (Aug. 1–3).
ϵ Perseids (Aug.).
μ Perseids (Aug.).
α Perseids (Aug.).

This is apparently a preferable method to that of Perseids I, II, III, IV, and V, which must occasion endless trouble in references to find what special stream is meant. Moreover the numbers seem only in fair application when affixed progressively to the

successive showers of the year, for it would be hardly consistent to call a radiant visible in *Perseus* early in January by the designation of, say, 'Perseids XXXVIII.' Yet this is what we are

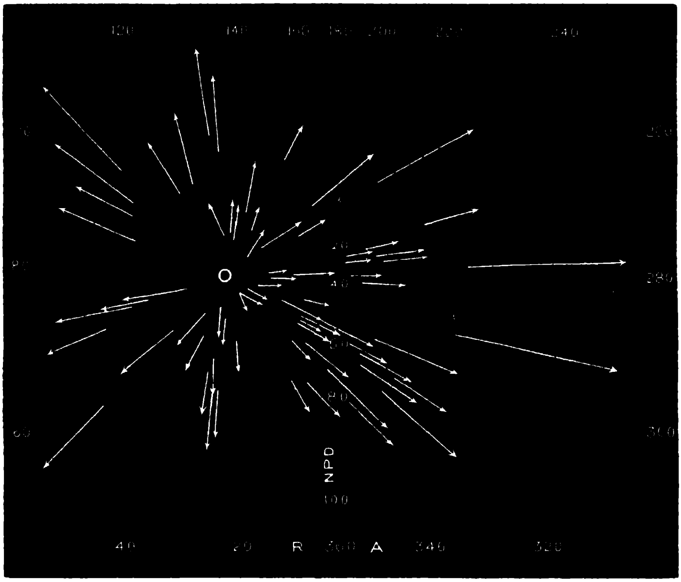

Fig. 5.—Shower of ε Perseids (61° + 36°) max. Sept. 6–7.

drifting to, unless a fresh system is introduced to accommodate the rapidly increasing number of meteor streams.

A LARGE CRATER.

By Prof. JOHN MILNE, F.G.S., Japan.

THE crater I wish to describe is called Asosan. It is situated in Kiushiu, the southernmost large island of the Japanese empire. The width of this crater is about fifteen miles, and in the bottom of it there are living, and living for aught I know in peace and plenty, about 20,000 people.

The way in which I came to find this crater was this: for the last four years I have been spending my spare time in travelling about Japan geologizing and visiting volcanoes. At the end of the summer of 1878, the only island of Japan which remained for me to see was Kiushiu, and a part of this I made up my mind to travel over during the coming Christmas holidays. After making application for extra leave, which was very kindly granted to me, I left Yokohama *en route* for Nagasaki, which for foreigners is the chief town in Kiushiu, on the afternoon of the 17th of December; the greater portion of this journey, which can all of it be performed in a comfortable steamer, is down what is called the Inland Sea. This journey is a trip which is made by everyone who visits Japan, and rather than being a journey down a sea, as its name implies, it is more like a journey along a river or a lake, which in places is so thickly studded with little islands, that at every moment they seem to come within a stone's throw. After a day's rest at Nagasaki, where I was joined by Mr. A. Wooley of the British Legation, I took a small steamer bound from Kumamoto to the capital of Hizen, one of the chief seats of the fighting during the last rebellion. At this point I may say that the luxuries of travelling were ended; and not knowing that there was anything particularly interesting before me, it was not with feelings of pleasure that I left the steamer and took to muddy roads and Japanese hotels. Foreigners who only come to Japan for a week and travel along a well-frequented road, or in the neighbourhood of an open port where the habits of the strangers from afar are more or less

understood, write descriptions of the empire of Japan sufficient to induce the whole world to come and settle in it. If, however, these gentlemen had been compelled to travel for say two consecutive months upon Japanese food and in districts somewhat remote from the great capitals, I think, if they ever lived to tell the story, that they would write a different account from those which we usually read,—when it becomes an absolute necessity to walk over mountain-passes which are more like flights of steps than ordinary roads, to sleep on hard mats, and to subsist on vegetable diet, the chief portion of which consists of rice and a radish called 'daikon,'—the hardships of travelling in Japan will be fully recognized. Occasionally a foreigner will endeavour to travel as a native; but usually, after finding that his health has suffered, you will discover him to have fallen back upon the usual plan of travelling, the essential part of which is to have a native servant and a pack-horse to carry some provisions; and under these circumstances it is but seldom that you can find a foreigner who is able or who cares to continue his exploration beyond the period of one month. At the end of that time, having lost ten per cent of his original weight, and longing for the flesh-pots of the open ports, he beats a quick retreat from the rugged hills and brawling streams, the enjoyment of which is hemmed in by so many difficulties.

From Kumamoto, where, in the new town, the ruined castle, and groups of troops, I saw indications of the recent war, I travelled directly eastward along a road which upon the native maps appears to lead from one side of the island to the other. Straight before us we could see Asosan, the mountain to which we were going, giving off heavy clouds of steam; between us and this there was a long range of rugged hills parallel with the coast which we had just left behind us; these looked reddish and bare, but when we came actually upon them, I found that their colour was due to a covering of brown grass, and not to earth and stones, as I had previously supposed. The road on which we travelled was, for a Japanese road, very wide; on each side of it there were two lines of trees, the lines nearest to the road were wax-trees, whilst those behind them were cryptomerias. As the wax-trees had lost their leaves, they looked very bare and ragged, but in summer time, when they are in full foliage, they must form an avenue which I think would far surpass anything I ever saw in an English park. Roads bounded with lines of tall trees are a feature in Japan, and some of these which continue for twenty or thirty miles in almost unbroken lines, form sights which when once seen will always be remembered.

After eleven and a half miles up this road, we reached the

village of Odzu, where we took up quarters for the night. Early next morning we started out upon frozen roads to climb the hills which were before us. The ascent was gentle. Right and left were broad stretches of uncultivated grassy ground. Away upon our left we could see a high mountain called Kurutake, which, from its general shape, and a rugged-looking hollow which had been breached upon the side towards which we were looking, seemed to represent the remains of an old volcano. Looking back, we could see the plain across which we had come on the previous afternoon; at the edge of it where it reached down to the sea, we could just make out the position of Kumamoto; whilst beyond that, at the other side of the bay on which Kumamoto is situated, there rises a rugged mass of mountains, the highest peak of which was the volcano Unsen. This volcano is the one which, amongst all Japanese volcanoes, has probably been the most destructive.

FIG. 1.—Ring Wall of Asosan as seen from Futaiyai-no-toge.

1. Dobindake. 2. Asosan (smoking). 3. Nekodake. 4. The Ring Wall. 5. Yudake. 6. The Crater Plain, with villages, clumps of trees, and rice-fields.

In 1793, during an eruption which extended over many days, a large portion of it literally blew up. The earthquakes that accompanied this outburst—the rushing in of the sea, and the falling boulders and fiery rain of red-hot cinders—laid waste the surrounding country, and took away the lives of fifty thousand of its inhabitants. The scenes which occurred during this eruption were too horrible for description, and, as a Japanese historian remarks, the terror and the ruin were unparalleled.

Turning round, and continuing the ascent, after a little more climbing we reached the top of the ridge called Futaiyai-no-toge; and here, before us, was a sight which was as striking as it was unexpected (fig. 1), because the ascent from the sea up to this point had been so gentle, being indeed only about 1750 feet. We had naturally expected that on reaching the

summit we should have before us a descent equally easy, but instead of that we found ourselves standing on the edge of what was nothing more or less than a deep pit, which, so far as we could see, was nearly circular. The greater portion of the sides of this pit were perpendicular cliffs of rocks, which here and there, near their upper parts, showed the irregular, broken stratification, so characteristic of the sides of many craters. In places at the foot of these cliffs a sloping talus had been formed; whilst in other places (which, I may remark, were few in number) the cliff-like forms had been so far denuded that the sides of the pit formed irregular, but exceedingly steep, slopes. Looking at this pit from the commanding position in which we stood, I estimated its width at seven miles; and it was not until we descended, and tried to walk across, and found how little was the progress which we made, that we recognized how far we had underrated its true dimensions. In the middle of the pit, and running up far above its sides, there is a large, irregular block of mountains, the central peak of which is always giving off large clouds of steam. This peak was Mount Aso, the goal of our journey. From the rim upon which we stood, by a zig-zag pathway, we quickly made the descent to the crater plain below us. The depth at this point was about 600 feet.

At the foot of these mountains the priests have their permanent rendezvous, and on the summit small temples and shrines, where during fixed seasons they reside, and receive the crowds of pilgrims to the deities of the mountain. The number of pilgrims who ascend the famous Fujiyama every year must be many thousands, and the fees the priests derive thereby, from the toll-gates on the upward paths which they have established, are very numerous, and must form a considerable revenue. If you visit some of these mountains at any other time than the appointed season, you may be refused permission to ascend. I myself was refused in this way at Iwakisan, one of the most beautiful volcanoes in northern Nipon. On another mountain, Chokaisan, I was subjected to a most curious treatment. I commenced ascending this mountain, and after scrambling over blocks of lava, and up long fields of snow, I reached the top, faint and weary, at half-past one o'clock p.m. My first impulse was to eat and drink, but in this I was prevented by four priests, who insisted that before satisfying either my hunger or my thirst I ought to pay my devotions at a small shrine which they had built. Being too tired and feeble to resist, I allowed them to lead me into the shrine, where I dropped on my knees before the idol between two priests, who, after putting on their robes of office, commenced to invoke the deity, and beat small drums. After this, they opened a small door in front of

me, and showed me my reflection in a metal looking-glass, where I suppose I was expected to see the lines which sin had graven on my face. Next, one of them handed me a large, clean, metal bowl. Instinct told me that an opportunity was coming to satisfy my thirst; so I took it reverentially in my two hands, and the priest immediately filled the bowl up with Japanese wine (*saki*), which I learnt afterwards had been dedicated to the gods. Never did nectar taste so good. After the first half pint the priest invited me to more wine, and, feeling faint, the offer was readily accepted. Again the offer came, but this was too much; modesty overcame me, and putting down twenty cents as an offering to the gods, I withdrew to my sandwiches. This was a Japanese sacrament, and I must say that I found it very good. The question now comes, What does all this mountain-worship mean? The reply to it I think we find in Buckle, who shows us how the imagination of a people has been excited by all great natural phenomena, especially those like earthquakes and volcanoes. The terror which a volcanic eruption has caused, like that at Unsen, when fifty thousand people were slaughtered almost in one night, we have historical evidence to show has been the cause of many superstitions. The phenomena were so terrible, so unexpected, and at the same time so inexplicable, that to account for them superhuman agencies were invoked and gods created. In Italy and Spain it would seem that it is to these seismic and volcanic agencies that we are in a great measure to attribute not only the superstitious character of the people, but also their poetry and arts. In Japan, however, the most prominent result of these terrible catastrophes appears to have been the cultivation of superstition. Not only has the religion probably been to a great extent an outcome of the phenomena of nature; but if we examine into their literature, and observe their sentimental reverence for antiquity, and the *conventionalities* in their art, we shall see that much of what is so peculiar in the national character of the Japanese may probably find an explanation by looking in a similar direction. This subject, however, is too large to dwell upon here.

From the foot of the crater to Bojo I calculated the distance to be about five miles, and as this point was about half way across this portion of the pit the total width would here be about ten miles. From a map of the crater, which our host, who kept a small shop in Bojo, made for me, the diameter in some directions must be fourteen or fifteen miles. This I confirmed by sketching in the position of the crater upon a map prepared by the government. Looking on the map, inside the space I marked out as being the boundaries of the crater, I counted about eighty villages. Fifty of the villages, our host

said, were a moderate size. If these contained say on an average three hundred people, then living in the crater there must be from fifteen to twenty thousand people.

Airy, in his *Popular Astronomy*, tells a story of an early philosopher, who, when writing a paper for the Royal Society intending to prove the truth of the Copernican theory, commenced for some reason or other with the assumption, 'Now we all know that hell is in the centre of the earth.' If this assumption is true—and it emanated from a member of a very learned body—the twenty thousand people I have referred to are perhaps unwittingly living upon the lid of that establishment.

The following account, which was given to me, of the last eruption of Asosan, might, in the mind of that early philosopher, have helped to strengthen his hypothesis:—'During the winter of 1873 sounds were heard, and white and black smoke was observed proceeding from the top of Asosan. On the 27th February in the following year, whilst the wind was blowing from the south, the ground began to quake, and ashes were thown out. What the thickness of the beds of ashes in the rice-fields was we cannot tell, but near to us they obtained a thickness of one inch. The ashes covered everything, and the leaves of the pine-trees and the wheat were turned quite red. At six o'clock in the morning of the 13th the ground again began to shake, and noises were heard on an average a hundred times an hour. On the 14th, at six o'clock, there were two or three very heavy shakes, and on the 23rd these became still more violent. These shakings were so strong that neither old nor young could sleep. They continued on the 24th, but on this day the eruption ceased. The material which was thrown out was of a grey colour, but afterwards it became red. The greatest quantity of ashes fell at Kurogawa-gumi and Higashi-kurogawa. At the commencement of the eruption, which was on the 1st of December, 1873, the volcano threw out stones one and two feet in diameter; and four men, who were working at some sulphur deposits on the top of the mountain, were immediately killed. Many hot springs burst out, and so much sulphurous matter was thrown into the river Shirokawo, which flows from this crater to Kumanoto, that all the fish were poisoned. Even up till the 3rd of March, 1874, shocks were felt, and material was thrown out which covered the ground for a distance of eighteen miles. During the day it was at times as dark as night. Previously, in 1806, there had been another serious eruption. The fame of this mountain spread even to China, and in a Chinese book I found the following:—

'Smoke rises up to the sky from mount Aso in Nipon. People say that in this mountain there is a precious stone of

a blue colour and shaped like an egg, which shines at night. They worship this, and call it Antikokusan. The shining smoke on the top of this mountain has three colours, which can be seen from a distance of three miles; these three colours are blue, yellow, and red.'

On the morning after reaching Bojo we started off to ascend the central peak of Asosan. After a climb of about 200 feet we turned round to look at the crater which we were leaving. At our feet was a cultivated plain dotted over with clumps of trees and villages, beyond which there was a line of fir-trees and Cryptomeria. These formed a belt round the foot of the amphitheatre of perpendicular cliffs which intercepted any further view. Before us, but on the left, there was a rugged peak called Nekodake, a portion of which looked very like a ruined crater. To the right and to the left of us was a wide expanse of sloping ground covered with brown grass. When we were 400 feet above Bojo we came to patches of snow. As we neared the top we crossed one or two old lava streams and beds of ashes. At the height of about 2000 feet above our starting-point, or about 3600 feet above the sea, we were on a level with the upper crater of Asosan, a huge black pit which was giving off vast clouds of steam. All the rocks which I saw up to this point were andesites, similar to those which form the ring-wall of the outer crater. Here we found one or two men who were engaged in collecting sulphur. Upon our right there was a rounded hill called Dobindake, which rose almost 500 feet above the level of the crater. The extreme height, therefore, of Asosan above the sea-level is perhaps a little over 5000 feet. From this position we had a good view of the big crater which surrounded us, as the slope on its outside is generally so gentle that it looked like a huge pit with perpendicular sides which had been dug out of the top part of a piece of ground in shape like an inverted saucer. On the northern side the cliffs which bound this pit are almost everywhere perpendicular; but on the south side, which was the side towards which we descended, they were more worn away to form rugged hills. The cliff-like character, with its horizontally-stratified structure, could, however, be in many places distinctly traced. That night, foot-sore and tired, we reached a village called Kurokawa. The only lodgings we could find were in a school-house, where, after a supper of biscuits, we shivered all night, lying upon the boards with our top-coats to cover us, our arms for pillows, and the thermometer somewhere below zero.

Next day we left the crater, passing through a breach in its north-west side. It is through this opening that the Shirokawa flows, the river which with its tributaries drains the crater plain.

Six days later, after walking many miles along roads which were often nearly a foot deep in mud, subsisting mostly on a poor vegetable diet, sleeping continually in our top-coats, caps, and all the clothes we could command, and experiencing snow, and rain, and hail, we were once more back enjoying the luxuries of Nagasaki.

Now, how does the crater of Asosan compare with other craters in the world? Amongst those which are active it appears to be the largest which has hitherto been discovered, and even if we include those which are extinct, it appears to take the foremost place.

Scope, in his valuable work on Volcanoes, amongst other remarkable craters, speaks of the following:—

In St. Helena there is 'a trachytic volcano encircled by a broken ring of basalt, the area of which measures eight miles by four.'

In the Mauritius there is a crater the shortest diameter of which is thirteen miles.

In St. Jago (Cape de Verde Isles) there is a similar crater.

The Cirque of Teneriffe is eight miles by six.

Pantellaria (near Sicily) has traces of a crater twelve miles in diameter. The rock is trachytic.

Bolsena (an oval lake basin) twelve miles in diameter.

Papandayang (Java), here there is a hollow fifteen miles by six, supposed to have been formed by the blowing off of the entire summit of a mountain by long-continued explosions.

Bromo (in Java) is a crater four or five miles in diameter, with perpendicular sides a thousand feet in height.

Another point of interest about a mountain like Asosan, and one which would form food for the speculation of almost every visitor, is the question as to how such a crater has been formed.

If I read Mr. Scrope aright, I must imagine that over this crater there was once a volcano, the upper portion of which, by a series of violent explosions, has been blown to atoms. In many cases the origin of craters in a manner like this is no doubt true, and this I may say, not because I should myself have imagined it, but because competent witnesses have seen the operation actually performed. In certain cases, however, and certainly in the case of a crater like that which we see at Asosan, I should be inclined to modify the suggestion of such paroxysmal causes and adopt something more gentle, and which to me seems more in accordance with the facts.

If from the heights and distances which I have given we make a drawing of Asosan, it will be found that the average slopes in the outside of the large crater must be about six degrees, and if we were to continue this slope upwards, we obtain a representation of the portion which, if it ever existed, has been

blown away. If such an occurrence has ever taken place, I find by calculation that we have to account for thirty-five cubic miles of material which represented the cone, and about seventeen cubic miles representing the crater, or in all fifty-two cubic miles of material which have disappeared. If the action was paroxysmal, surely some of this material ought to be found in blocks and boulders, distributed round the outside of the big crater. Although I crossed the outside mountain in two paths, I must say that I failed to meet with such material, in fact I do not remember seeing even a single boulder, all was smooth.

If in spite of this we still hold on to the idea of paroxysmal actions, the only refuge which we have seems to be that the whole of this vast quantity of material was suddenly dissipated as dust. A conclusion of this sort seems to me improbable; and instead of regarding this crater as the basal wreck of some large mountain, I should be inclined to look upon it as being now as it ever was, the upper crater of an old volcano, inside which in more recent times a cone has grown. Although at the commencement of the mountain the action may have been cataclysmic in its nature, subsequently, however, I should think that it grew up higher, partly by the accumulation of ashes, but now perhaps by the boiling over of a highly liquid trachytic lava. That this latter action has taken place seems to be testified by the roughly stratified appearances which are exhibited in the ring walls; the growth has, in fact, been probably something like the growth of Mauna Loa in the Sandwich Islands, or a geyser tube in Iceland. I may also add, that were we to suppose the upper portion of a mountain like that which must have existed if we complete in our imagination the truncated remains which bound large craters such as Asosan to have been blown away, we are, I think, assuming that the later eruptions of these mountains were more powerful than the first, whereas, I think, experience teaches us that the reverse is generally the case, as the action of a volcano continues by the quantity of material it piles upon itself. The hydrostatic pressure of its new column of lava, the increase in size of the cavity produced by evisceration in which we may suppose the actuating steam to be confined, are causes which will all help to make succeeding outbursts vigorous. No doubt examples might be quoted to show the reverse of what is here suggested, but I think that many more examples might be collected to show its truth; and certainly if we could regard volcanic energy as a whole through all past times, the enfeeblement in volcanic energy which has taken place would be fully recognized.

Amongst the large craters which I have mentioned, and

these are the largest which are recorded by Scrope, it would seem that Asosan, considering its size and its activity, is without a rival. If we go further, leave the earth and compare Asosan with craters which we find upon our satellite the Moon, although it cannot stand before a pit like that exhibited by Copernicus, which is said to have a diameter of fifty-six miles, it nevertheless may be regarded as an example of healthy competition.

As an active volcano, however, it still holds its place; and if America boasts of the largest waterfall, and India of the highest mountains, in one of the most prominent classes of natural phenomena Japan, also, will be able to take an equally prominent position. Further explorations may perhaps lead to the discovery of craters which will excel Asosan; but so far as present knowledge is concerned, amongst the active craters in the world, as yet Asosan appears to be pre-eminent.

And now not only may the Japanese boast of possessing one of the most beautiful of volcanoes, which mountain is the far-famed Fuji, but they may boast of one of the most remarkable of craters.

REVIEWS.

FRESH-WATER RHIZOPODS.*

WE often enough hear of the great effects produced in the world by the smallest organisms; and one of these seems to be their giving origin to big books. Ehrenberg's *Infusionstheichen* and *Mikrogeologie* are large folios, and many authors have devoted stout quarto volumes to the publication of their researches upon Infusoria, Foraminifera, and Radiolaria. To this number we have to add Prof. Leidy, who has just brought out a magnificent volume upon the fresh-water Rhizopods of North America. This work is a posthumous publication of the defunct Geological Survey of the Territories, which, under the able superintendence of Dr. F. V. Hayden, did so much admirable work in various departments of natural history; and we are glad to see that, notwithstanding the change that has taken place in the arrangements of the United States Surveys, the investigations inaugurated and carried on by Dr. Hayden and his assistants are not likely to be lost to the world.

Prof. Leidy has devoted several years to the investigations of which the results are published in the work now before us. Besides collecting assiduously in the vicinity of Philadelphia and other parts of Pennsylvania, in New Jersey, Connecticut, Rhode Island, and Nova Scotia, on the Atlantic side of the continent, he has devoted two seasons to an examination of the forms inhabiting some of the far distant regions of the west—especially the Bridger Valley, in the south-west of Wyoming, and the Uinta Mountains, in the same region. In the last-mentioned locality he pursued his researches up to an elevation of 10,000 feet.

Of course it is unnecessary to say that such investigations do not by any means make us acquainted with all the forms of the groups here treated of which may be found within the vast territories of the United States, nor does Prof. Leidy claim to have even approximately exhausted his subject.

* *Report of the United States Geological Survey of the Territories. Vol. XII. Fresh-water Rhizopods of North America.* By Joseph Leidy, M.D. 4to. Washington: Government Printing Office, 1879.

His object, and that of Dr. Hayden, in the production of the work, was to show observers in the States what a wealth of interesting objects awaited their investigation, and to furnish a trustworthy guide-book which would enable them to know what had already been done.

At the same time, we think that the author has indicated (not only here but in scattered papers published during the progress of his researches) that these lowly organisms are much more generally distributed than was formerly supposed. He says: 'Freshwater Rhizopods are to be found almost everywhere in positions kept continuously damp or wet, and not too much shaded. They are especially frequent and abundant in comparatively quiet waters; clear, and neither too cold, not too much heated by the sun . . . . . . They are also frequent in wet bogs and savannas, among mosses, in springy places, on dripping rocks, the vicinity of waterfalls, springs and fountains, and in marshes, wherever the ground is sufficiently damp or moist to promote the growth of algæ. They are also to be found in damp, shaded places, among algæ, liverworts, and mosses, about the roots of sedges, rushes, and grasses, or those of shrubs and trees growing in or at the borders of bogs and ponds, or along ditches and sluggish water-courses. They are likewise to be found with algæ in damp, shaded positions in the depressions and fissures of rocks, in the mouths of caves, among decaying logs, among mosses and lichens on the bark of growing trees, and even in the crevices of walls and pavements about old dwellings, and in cities.' * And to encourage the student to work upon this abundant and almost ubiquitous material, he adds that it is a mistake to suppose that a very elaborate microscope is necessary for its examination, such an instrument as may be procured for about 10*l.* being generally sufficient for every purpose. We may add that he gives full directions for collecting the animals in various localities.

Dr. Leidy regards the Rhizopoda (which he accepts in the old sense of the term) as including five orders, namely, Protoplasta, Heliozoa, Radiolaria, Foraminifera, and Monera. Of the last he candidly admits that he knows no freshwater form, unless *Vampyrella* (here referred to the Heliozoa), which Hackel places among his Monera, really belongs to that group. The Radiolaria are marine; of the Foraminifera our author admits one freshwater type, *Gromia;* and the rest of the forms described by him belong to his orders Protoplasta and Heliozoa. The Protoplasta includes the whole of the Amœboid forms, whether naked or shelled (*Protoplasta lobosa*), as well as the Euglyphoid types (*P. filosa*); to the Heliozoa are referred *Actinophrys* and *Actinosphærium* with the allied forms which have been made the types of distinct genera by various authors, and also, as already stated, the genus *Vampyrella* of Cienkowski. The only representative of the genus *Gromia*, described as a new species (*Gromia terricola*), was found with Rotifers and other organisms among moist moss in the crevices of pavement in a yard in Philadelphia.

It is impossible to estimate too highly the care and labour that Prof. Leidy has bestowed upon the preparation of this splendid work. All the forms discovered by him are described in the most careful manner, as regards both their form and structure, and their habits; and the opinions of authors

* P. 8. Lists of forms obtained together will be found at pp. 289–293.

on this side of the Atlantic are cited and discussed, where necessary, in great detail. Taken in conjunction with the beautiful series of forty-eight coloured plates with which the descriptions are illustrated; this portion of the work, as a means of distinguishing the organisms, and as a 'recreation of the eye,' as old Knorr has it, leaves little to be desired. But we cannot help thinking that the author would have done better, and have given his work a far higher value, had he taken a rather more philosophical view of what is wanted in the shape of genera and species in the classification of these lowly organisms. The multiplication of generic names especially is a continual stumbling-block to everyone but the mere specialist; and when we examine the beautiful figures here given and compare them with the observations and figures of Dr. Wallich (*Ann. Mag. Nat. Hist.* 1864), one is driven perforce to accept the views of the latter distinguished observer, and to hold that, for the purposes of science, the establishment of species and genera upon slight variations in the form and structure of little cases like those of the *Difflugiæ* and *Euglyphæ*, is worse than useless. Nay, Prof. Leidy himself, evidently holds a very similar opinion, for he cites the authority of such observers as Carpenter, Williamson, Wallich, Brady, Parker, and Jones to show 'that the members of the class are infinitely variable, and that indeed no absolute distinctions of species and genera exist, such as appear more definitely to characterize the higher forms of animal life.' He adds: 'My own investigations rather confirm this view, and, under the circumstances, we can hardly regard the more conspicuous and prevailing forms as so many nominal species, in likeness with the species of higher organic forms, more or less intimately related, and by intermediate forms, or varieties, merging into one another' (p. 6). This seems very nearly to express the state of the case; but because there are no well-marked or easily distinguishable groups, it by no means follows that it is of no consequence how many groups we adopt.

In these remarks we have no wish to detract from the value of Prof. Leidy's magnificent work, which is not only a splendid monument of much labour carefully and intelligently performed, but also a most important and indispensable book for the investigator of the lowest forms of animals. We congratulate him heartily on its completion, and on the exceedingly satisfactory manner in which it has been produced.

METEOROLOGY.*

There has lately been an addition to the Literature of Meteorology in the shape of two pamphlets, each of which merits some notice. The first issued officially by the Meteorological Office is entitiled *Aids to the Study and Forecast of Weather;* and the second by E. J. Lowe, F.R.S., is called *The Coming Drought, or the Cycle of the Seasons, with a Chronological History of all the*

* *Aids to the Study and Forecast of Weather.* By W. Clement Ley, M.A. 8vo. London: Bemrose & Sons. 1880.

† *The Coming Drought, or the Cycle of the Seasons.* By E. J. Lowe, F.R.S. 8vo. London: Bemrose & Sons. 1880.

Droughts and Frosts as yet found recorded from A.D. 134 *to the present Time*, a title that irresistibly reminds the reader of the black-letter ballads of some centuries ago. Both books have one object, the very important one of showing how in our very variable climate the weather may be foretold; but the scope, the matter, and the investigations are very different in the two cases. In Mr. Lowe's pamphlet we have information extending over centuries, observations collected from all quarters of the globe, the results generalized, and a large margin given; while Mr. Ley's work contains all the minutiæ of the science, the small meteorological phenomena of every-day life being there noted and explained. Neither work pretends to greater distinction than that of being a short sketch of the subjects of which it treats; and in the case of Mr. Lowe's book, it is avowedly an introduction to a more important work which is now in course of preparation.

The '*Aids*' is divided into three distinct parts. It deals first with the most important non-instrumental observations to which attention should be given by the student of the weather; secondly, with the relations which exist between the winds and the distribution of barometric pressure; and thirdly, with the conditions of weather which attend and characterize atmospheric disturbances.

With the exception of a few words as to 'backing' and 'veering,' wind observations are shortly dismissed, and the author passes on to the subject of Clouds. To this subject he has evidently given his closest attention, and it is evident from his remarks that by closely watching the varieties and movements of clouds, good guesses as to changes of weather can be made even for two or three days in advance. Other weather signs are then discussed by the author, and the various proverbs, 'The observations of the many set forth by the wit of one,' are scientifically explained, and either confirmed or refuted. It would, in our opinion, have been preferable had this part of the book been very considerably extended; instead of only eight pages out of thirty-eight being devoted to this popular and non-instrumental part, it might at least have been expanded into three times the space with a corresponding abridgment of Parts II. and III. Part II. deals with the relations of pressure and winds, not only in our immediate neighbourhood, but in all parts of the globe, and very pretty and interesting examples of the cyclones and anti-cyclones are given, together with the circulation of the winds round those centres; but throughout the whole of this part there appears to be a more or less general reproduction of the matter which was given to the world in Mr. Scott's Weather Charts and Storm Warnings, and the whole of this division pre-supposes the possession of the weather charts, and the time to study them.

Part III. gives us again a quantity of useful and popular information as to changes which may, and do actually take place over our heads; and at the conclusion specimens are given of the sort of forecast which an observer can make, even *without the aid of the weather-maps*, a desideratum which cannot be too strongly insisted on. At the conclusion, the author has given two maps of the mean atmospheric pressure and prevailing winds for the months of January and July, the principal phenomena in which are noted in the chapters on those subjects. Speaking about these charts, he says that we find areas of low pressure around the poles, but this does not appear to

be so in the January chart in the case of the North Pole; and the fact that north-east winds are reported from Iceland and Greenland, proves the existence of high pressure in the high latitudes.

Turning to Mr. Lowe's pamphlet, we find that he starts with the assumption which every meteorologist seems so disposed to admit, yet finds so difficult to substantiate, viz. that a cycle of the seasons exists, though its precise period has yet to be ascertained. Mr. Lowe himself is satisfied that the cycle is about eleven years, thus agreeing with the Sun-spot theorists (though throughout the book there is no reference to sun-spots, or to this agreement), while at the same time he considers that at every ninth cycle the phenomena are much intensified.

An extraordinary amount of trouble has undoubtedly been expended in the compilation of this work, and the authorities being quoted, an immense assistance is thereby afforded to any one continuing the investigations. That further investigations are needed is, unfortunately, most true, for we find that during the past six, centuries fifteen droughts were accelerated one year, and nine were retarded one year, while six were accelerated two years, and four retarded a like period. This compels us, in giving a forecast, to allow a margin of four years for possible eventualities, a margin which is much too large to give the forecast a practical value. This value may, however, very shortly be tested, as Mr. Lowe considers that with last October there commenced three years of drought and frost, a forecast which every succeeding day will go to prove or refute.

HEAT.*

PROF. TYNDALL'S well-known treatise on the mechanical theory of heat has now been out of print for a considerable time, so that the appearance of this sixth, revised and enlarged edition, will be welcomed by many students. Founded as it is upon his lectures delivered at the Royal Institution, and retaining the lecture form in its chapters, while the numerous experiments which the Professor delights to bring before his audiences are copiously illustrated by figures of apparatus in use, the style in which the information is conveyed to the reader has a freshness and vigour about it hardly attainable by any drier mode of treatment, and one is at no loss to understand the great popularity that this book has so long enjoyed. In his new edition Prof. Tyndall has evidently been careful to work in the most recent results of physical researches in the somewhat wide field that he undertakes here to open up to his readers, his book, as is well known, discussing a host of phenomena with which heat is more or less immediately concerned.

* *Heat a Mode of Motion.* By John Tyndall, D.C.L., LL.D., F.R.S. Sixth Edition. 8vo, London: Longmans, 1880.

FLOWERS.

WE have received two little books intended to aid the student during his earlier steps in the practical study of botany. One of these is a pamphlet* for the use of beginners, and for teachers in schools, arranged somewhat on the same model as a small book by Mr. Henslow, which we noticed some time since. The author, Dr. Andrew Wilson, has selected the following illustrations of floral structure,—the buttercup, wallflower, primrose, apple, dead nettle, tulip, daffodil, iris, pea, and daisy; and his explanation of the structure of these, and of the mode in which it may be investigated, is exceedingly simple and clear. In a concluding section he notices the conditions which modify flowers, and gives examples of schedules for filling up with the characters of those examined by the student, followed by a brief lesson on the general physiology of the plant, which contains a great amount of useful information in a very small space.

The second book † is of a more ambitious character, and is intended for the behoof of more advanced students. It consists essentially of a series of analytical tables of the natural order and genera of British flowering plants and ferns, arranged on a peculiar principle:—The left hand page in each opening contains a table of symbols indicating the floral character, and those of the leaves and fruit, with the name of the order or genus thus illustrated; on the opposite page we find brief characters of the respective groups, with the English name of the type form. These tables are preceded by a short glossary of terms, and followed by a table showing the numerical strength of the different natural orders in the British flora, and by a catalogue of the species of British plants.

It is a somewhat curious experience to be set to read the characters of plants by means of a system of picture-writing, and in examining the book one feels at first a little puzzled to know what some of the symbols mean; but on further examination the rough places become smooth, and we have no doubt that Mr. Messer's book will prove a considerable boon to young students of botany. Of course the figures are all purely diagrammatic: but the book is very nicely printed and got up.

PURE AND APPLIED MATHEMATICS.‡

THIS is, for the most part, a carefully edited print of the note-book of a teacher of mathematics. Its leading idea is undoubtedly that of a repetition book for 'cramming,' and it seems to us that it is not only well adapted

* *Introduction to the Study of Flowers, being Practical Exercises in Elementary Botany.* By Andrew Wilson, Ph. D., F.R.S.E. 12mo. Chambers: London and Edinburgh, 1880.

† *A New and Easy Method of Studying British Wild Flowers; Being a Complete Series of Illustrations of their Natural Orders and Genera, analytically arranged.* By Frederic Messer. 8vo. London: David Bogue. 1880.

‡ *A Synopsis of Elementary Results in Pure and Applied Mathematics.* By G. S. Carr, B.A. London: C. F. Hodson & Son. Vol. I. part i. (xxiv. and 256).

for this its main purpose, but that it would be doing it some injustice to call it a mere cram-book. The author's notion was to supplement the use of the ordinary text-books, with the view of assisting the student in the task of revision of book-work, and, in the author's own words, 'To this end I have, in many cases, merely indicated the salient points of a demonstration, or merely referred to the theorems by which the proposition is proved. I am convinced that it is more beneficial to the student to recall demonstrations with such aids, than to read and re-read them. Let them be read once, and recalled often.' This is a sound view of mathematical teaching, whether the object of that teaching be a mere cram for a tripos, or the preliminary grind which is necessary to all high reading.

Besides this, the author has endeavoured to make the book a general *aide-mémoire*. In this we think he has not been quite so successful, the scholastic requirements of arrangements interfering not only with the facility of reference, but also governing the subject-matter, especially in respect of that concreteness which is so necessary for practical uses, and so much out of the direct line of usual instruction. As a work of reference it is not so well planned as another Synopsis by another Mr. Carr, of which a second edition was published by Weale in 1843.*

With a view of making the book one of reference, there has been prefixed to it a chapter containing an account of the centimètre-gramme-second system of units, and some mathematical tables, chiefly in very short abstract, but with two tables fully given—a factor table from 1 to 99,000, reprinted from Burckhardt, and a table of the Gamma function, reprinted from Legendre. This set of tables does not appear to us to be very well selected or arranged, nor to be of much utility. Moreover, as the first volume is concerned exclusively with pure mathematics, the physical units are rather out of place in it, and might advantageously have been reserved as an introduction to the applied mathematics which are to be included in the second volume.

The part before us includes algebra, the theory of equations, and determinants, plane and spherical trigonometry, elementary plane geometry, and geometrical conics. Of these it seems to be a well-selected synopsis, well arranged too for its chief purpose as a book of repetition, and containing a good number of useful theorems, especially in the algebra, which are not usually to be found in the books of elementary reading. Among them we note Gauss's resolution of a hypergeometrical series into a continued fraction, and various theorems upon equations and determinants.

The type, and paper, and the general 'get-up' of the book, are not at all to our taste. It is, however, clearly printed, and there is a good table of contents. On the whole, if the work, when complete, is supplemented by a good index, it will occupy a place not filled up at present.

* There is a curious story about this book. It was printed in Durham some years before the above-mentioned date, and soon became out of print and scarce, although it was known that 5000 copies had been struck off. In 1842 the stock of a London publisher came to the hammer, and among the remainders were found, in sheets, 3500 copies of the work. The re-issue of these formed Weale's second edition.

THE INDUSTRIES OF INDIA.*

ON the recent disintegration of the collections which were contained in the East India Museum, all the botanical specimens were transferred to Kew, while the zoological, the mineralogical, and the architectural specimens passed to the British Museum. There remained, however, at Kensington a vast number of valuable objects illustrating the arts and industries of India, which formed in fact the most popular part of the old Museum. These objects have been transferred from the India Office to the custody of the authorities of the Science and Art Department, and they are consequently now exhibited as a part of the South Kensington collections. The transfer was primarily effected for the purpose of relieving the Indian Exchequer of the cost of maintaining a museum in London; while under the new arrangement the guardianship of the collections throws no additional burden upon the Imperial Exchequer, since it has been undertaken by the staff of keepers who already had charge of the South Kensington Museum. As soon as the Kensington authorities obtained possession of the collections they commissioned Dr. Birdwood, of the India Office, to prepare a Handbook which should form one of the series of works on art in course of issue by the Department. The result of Dr. Birdwood's labours is seen in the interesting work now in our hands.

So closely are the arts of India interwoven with the various religious beliefs of the people that no thorough appreciation of native art is possible without some familiarity with Hindu mythology. The author has, therefore, devoted the early part of his work to a description of the Hindu pantheon. In the second part he deals with the master handicrafts of India, principally with the metal-work, the jewellery, the pottery, and the textile products. This part is, to a great extent, an enlarged reproduction of the author's well-known *Handbook of the Indian Court of the Paris Exhibition of* 1878. It should be distinctly understood that the present work is not a mere Guide to the Indian Museum, much less a Catalogue of its contents. It is, in truth, a pleasantly-written book, full of information on the arts, the religion, and the social life of India—a work which may be read at home without the slightest reference to any particular collection of Indian products. In fact, if we are obliged to find any fault with so good a book, it is in the direction of discursiveness rather than of elaboration. Taking, for example, the chapter on Indian pottery we find that considerably more than half the space is devoted to a description of village-life and land-tenure in India. It should be added that Dr. Birdwood's volumes are copiously illustrated by excellent wood-engravings, but we regret that they are not furnished with an index.

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* *The Industrial Arts of India.* By George C. M. Birdwood, C.S.I., M.D. With Map and Woodcuts. 2 vols. 8vo. London: Chapman & Hall.

RAMBLES IN SEARCH OF MINERALS.*

A MOURNFUL interest clings to this little book as being the last work which issued from the active pen of the late Prof. Ansted. Perhaps no science is more difficult to popularize than Mineralogy, since its veriest rudiments cannot be understood without some scant knowledge of solid geometry and of chemistry. Notwithstanding the necessary exclusion of crystallographic and chemical expressions, Prof. Ansted has contrived to give intelligible descriptions of a number of minerals, and to convey to the reader in a pleasant gossipy way a great deal of information about their mode of occurrence. The volume forms one of a series entitled *Natural History Rambles*, but the rambles in this case must necessarily be taken rather far from home, since the principal part of the book is devoted to a popular description of gems and various ornamental stones which are not likely to be met with in any part of the British Islands. But if gem-stones be taken away, mineralogy loses nearly the whole of its popular element, and we are consequently not disposed to cavil at their intrusion into our *Natural History Rambles.*

GUTHRIE'S ELECTRICITY.†

THE critic finds his occupation gone as he reads on the title-page of this work that it has already reached the 'Fifteenth Thousand.' Such a sale is alone sufficient to show that Dr. Guthrie's text-book has been found widely useful to students of physics. Having used the work in class-teaching the present writer can testify without hesitation to its sterling worth. The work is based upon the lectures which the author has been in the habit of delivering in his annual course at the Royal School of Mines. It is to be regretted, however, that successive editions are not revised so as to keep pace with the advance of electrical science: we fail to find, for example, any reference in the Index to such subjects as the Telephone and Microphone.

THE SUPERNATURAL IN NATURE.‡

THIS very able book, published at first anonymously, has reached a second edition, and its success has determined the author to reveal himself and also to thank some well-known scientific men for their help. The object of

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* *Natural History Rambles: in Search of Minerals.* By D. T. Ansted, M.A., F.R.S. 8vo. London: Society for Promoting Christian Knowledge. 1880.

† *Magnetism and Electricity.* Collins' Advanced Science Series. By Frederick Guthrie. With 300 Illustrations. 8vo. London and Glasgow: Wm. Collins, Sons, & Co.

‡ *The Supernatural in Nature, a Verification by free use of Science.* By Rev. Joseph William Reynolds, President of Zion College. 8vo. London: C. Kegan Paul and Co. 1880.

the work is to reconcile science and Scripture; and it is written for the sake of the true lovers of science 'and other truth-loving men, who are in danger of being beguiled by the sophisms of an imperfect science.' And the author decides that physical science is the sister and handmaid of Revelation: that no lasting antagonism can exist between them, nor will man lastingly receive a religion that requires antagonism. He considers that science has not yet advanced far enough to establish perfect accord with revelation, but is tending thither; and when attained, the generalizations of science will no longer be doubtful but assured. His aim is to promote that agreement by showing the correspondence between truly scientific conclusions and Holy Writ; by exposing hasty generalizations which appear contrary to revelation, by making it plain that science is knowledge as exact as is possible to finite wisdom, and that scientific truths like the spiritual have for ever been descending from heaven to man. The author notices that the most brilliant scientific work of late years has been amongst the 'unseen,' and that the connection of all visible things with the invisible is plain enough. The multitude of inexplicables, the impossibility of comprehending ultimates, and the fact that scientific orthodoxy becomes scientific heresy with the progress of induction and observation, together with the vast unknown, all tend not only to make the true student of science humble, but also disposed to admit powers and energies environing everything and beyond the ken of nature.

The book is full of most accurate scientific statement, and the amount of physical, biological, and astronomical learning displayed is very great. No uncharitable remarks deface the work, and it is written in a grand style which often rises to great eloquence. There is no other book of the kind, and it must be most valuable to the preacher who will condescend to instruct his hearers on something else than dogma, and to the conscientious theological student.

LAND AND FRESHWATER SHELLS.*

MR. RIMMER has published an excellent little treatise upon the Land and Freshwater Shells of the British Isles, and one which will, doubtless, be found exceedingly useful by many young students. He tells us in his preface that he has 'followed the author of *British Conchology* in the method of arrangement, as well as in the nomenclature which he has adopted;' and on the same page he acknowledges his indebtedness to the author of that work, Dr. Gwyn Jeffreys, so that we can understand the close agreement, not only in arrangement and general treatment, but actually in the wording of the descriptive parts that prevails between the two books. However, the author could hardly have followed a better model, so that his readers have certainly no reason to complain.

* *The Land and Freshwater Shells of the British Isles.* With Illustrations of all the Species. By Richard Rimmer, F.L.S. 8vo. London: D. Bogue, 1880.

Mr. Rimmer has added localities for many of the species from his own observations, from those of correspondents, and from local lists published in journals; he has also in some cases added information on the natural history of the animals, and introduced in their proper places a few species discovered or admitted to specific rank, or admitted to British citizenship since the publication of the first volume of the *British Conchology*, so that his book may be taken as a good and trustworthy exposition of our present knowledge of the British territorial and freshwater shells. We notice one curious statement, however, an explanation of which would be desirable. The author says that the Sphæriidæ have the siphons placed in front, instead of at the posterior end of the body, an assertion which is rather startling.

Following Dr. Jeffreys' example, Mr. Rimmer has given the accentuation and translation of all the generic and specific names occurring in his book, and these occur again, together with the terms used in description, in a glossary appended to the work. The mode of pronunciation of the words is also indicated, in a very effective, if rather grotesque fashion.

The little book is illustrated with ten plates, giving figures of all the species, mostly reproduced from photographs by the 'Albertype process.' We must confess that, although some of the figures, especially of the larger shells, are very beautiful, and nearly all of them, except the minute ones, furnish a fair idea of the objects represented, we must still adhere to the conviction that we have entertained for many years, that photography is not adapted to produce satisfactory illustrations of natural history objects. Of two sets of species, namely, the slugs, on account of their restlessness, and the species of *Vertigo*, on account of the minuteness of the characters of the mouth, which had to be represented, the author has given good lithographed figures.

We hope that Mr. Rimmer's labours may be rewarded as he desires, by the diffusion among the people of a taste for investigating the natural objects which everywhere surround them in such profusion; and that he will soon find in his rambles that he is no longer regarded by chance passers-by as a harmless lunatic escaped from some asylum.

SCIENTIFIC SUMMARY.

ANTHROPOLOGY.

Remains of the Stone Age near Tyre.—M. Lortet has communicated to the French Academy (*Comptes Rendus*, 16th August, 1880) the results of some explorations made by him at Hanaoueh, a small village among the mountains east of Tyre, at a distance of about two hours and a half from that place, and close to 'Hiram's tomb.' To the north of this, upon a hill are the ruins of a Phenician citadel, investigated by M. Renan, and at its base winds a wild and barren valley (Wady-el-Ahkat) deeply cut into the thick deposits of a cretaceous limestone. Following the left wall of this ravine to the eastward, M. Lortet came upon some rock escarpments, which extend for a long distance, and upon them were carved in high relief a number of small statues, from 0·80 to 1 metre in height, having all the characters of a very high antiquity. A little way from these singular monuments at the foot of an abrupt artificial face, about 4 metres in height, there were enormous blocks of a reddish rock, so hard as to be almost unbreakable with the hammer. This rock proved to be a conglomerate or breccia containing myriads of worked flints, and numerous fragments of bones and teeth. The soil all round was strewn with roughly-worked flints, among which were points and scrapers of the so-called Moustierian type. The flints are yellow or black, and of very fine grain; those in the blocks are often exposed at the surface by the action of the weather, but the rock is so excessively hard that it is almost impossible to detach them from the gangue, and they break rather than separate from the cement that holds them together.

A few fragments of teeth extracted from this intractable matrix are considered by M. Lortet to indicate the genera *Cervus*, *Capra* (or *Ibex*), *Bos*, and *Equus*. M. Lortet remarks that this station seems to date from the most remote antiquity. The flints present a very primitive form, much more archaic than those found in the caves of the Nahr-el-Kelb, near Beyrout, and a long series of centuries would be necessary to give to these kitchen remains a hardness equal to that of the most compact porphyry. He thinks that this matrix must have been formed in a cavern, the roof and walls of which were subsequently removed by the Proto-Phenicians, who

worked the rude figures above mentioned. The breccia, being too hard for their tools, was left by them exposed as it is now found. M. Lortet does not, therefore, regard the makers of the flint implements and the artists of the statues as one and the same people; and he remarks that it is interesting to find here within a very limited space the traces of three races which have successively inhabited the country, namely, palæolithic man, the Proto-Phenicians, and the Phenicians of historic times, whose works abound in the neighbourhood.

An extra Bone in the Human Wrist.—Dr. Eugène Vincent has found an additional (ninth) bone in each wrist of an old Arab. The first row of carpals consisted, as usual, of four bones, but the second row had five; the supplementary bone, which was equal in size to the pisiform, was between the trapezium and the grande, and it was applied against the scaphoid above and the trapezoid in front, but articulated by one of its faces with the second metacarpal. The structure was the same in both wrists. The Orang and most of the lower Apes regularly possess a ninth bone in the carpus, but this differs somewhat in position from the bone found by Dr. Vincent, and does not appear to reach the second metacarpal bone which is nearest to it. In the Quadrumana, Cuvier considered the supplementary bone to be a separated portion of the grande; but according to the opinion of M. Alix it is rather a dismemberment of the scaphoid. Dr. Vincent regards the ninth bone in his Arab as probably derived from the trapezoid, which was much reduced in size.—(*Compte Rendu de l'Assoc. Franç.*, 1879.)

ASTRONOMY.

Secular Acceleration in the Mean Motion of the Moon.—At the April meeting of the Royal Astronomical Society, Sir George Airy read a paper on the 'Theoretical value of the Acceleration in the Moon's Mean Motion in Longitude, produced by the change of Eccentricity of the Earth's Orbit,' in which he advanced the opinion that the true value of this acceleration was really 10″·1477 per century, and not 6″·18 as generally supposed by astronomers. The true value of the acceleration in the mean motion of the Moon produced by the gradual diminution in eccentricity which the Earth's orbit is undergoing, is a question which was warmly discussed by astronomers some twenty years ago, but which has been considered definitely settled for the last fifteen years, so that Sir George Airy's paper created much surprise. The whole question is purely a mathematical one; it can only be settled by mathematical analyses, and is one about which there ought to be no doubt. The existence of a gradual acceleration in the motion of the Moon had been known for a long time, but the explanation of its origin had been sought in vain by astronomers, until it was discovered by Laplace that the diminution of the eccentricity of the Earth's orbit would produce such an acceleration. Laplace calculated its value approximately, omitting a number of terms which seemed too small to be sensible, and found it to be equal to 10″·1816 per century, which was in perfect accord with that which seemed

indicated by the observations, namely, 10″·3. The agreement was so close, that Laplace did not consider it necessary to push his calculations any further. Subsequently other astronomers did push the calculation further, taking into account a number of small factors omitted by Laplace, and found 10″·63 for the theoretical value, and 10″·8 for the observed value. In the year 1853, a remarkable paper was communicated to the Royal Society by Prof. J. C. Adams, and in this memoir it was clearly shown that these later astronomers had taken into account only a portion of the terms omitted by Laplace, and had entirely overlooked a most important class of these additional terms. Taking all these into consideration, Prof. Adams showed that the real value of this acceleration produced by the variation in the eccentricity of the terrestrial orbit, was only 6″·20. For some time Prof. Adams' paper did not excite the attention it deserved; but during the period 1858 to 1863, Prof. Adams' investigation was warmly discussed by all the leading masters of the lunar theory. In the end, the correctness of Prof. Adams' results were unanimously admitted by all, and independent proofs were furnished by, amongst others, M. Delaunay and Prof. Cayley. For the last fifteen years, therefore, it has been generally admitted by astronomers that Prof. Adams had conclusively established the correctness of his views. It is true that the researches of Prof. Hansen seemed to show that the observations indicated an actual acceleration of over 12″·2, but it was known that the whole of this acceleration did not necessarily proceed from the diminution in the eccentricity of the Earth's orbit.

Although Sir George Airy made no reference to Prof. Adams in his paper, it was obvious that the paper could only be regarded as a direct attack on Prof. Adams' views; for if Sir George Airy was correct, then Prof. Adams was as certainly wrong. Naturally, therefore, the publication of Sir George Airy's paper was waited for with impatience by astronomers, who were curious to see the grounds on which Sir George Airy had been led to impugn the correctness of the unanimous opinion of all the greatest authorities on the lunar theory. The paper was published in the beginning of May, and must have been read with astonishment by astronomers. The importance of this question is far too great to permit of any doubt being allowed to remain as to the truth, so that, although Sir George Airy disclaimed any wish to raise a controversy on the matter, it was absolutely indispensable that the real truth should be at once made known. Everyone looked, therefore, to Prof. Adams for a reply. This Prof. Adams lost no time in sending in to the Royal Astronomical Society, where it was read at the May meeting and published in the middle of June. In this reply, Prof. Adams points out that Sir George Airy has misunderstood the nature of the results which he had arrived at in his investigation; and that, instead of the value deduced by Sir George Airy being the complete value of the secular acceleration as he supposed, it was only a portion of the whole. Prof. Adams further showed that Sir George Airy had neglected as insensible, or else omitted as unimportant, all the very quantities which had been shown by Delaunay, Cayley, and himself to have so important an influence on the value of the secular acceleration; and that, in fact, Sir George Airy had, without suspecting it, practically arrived in a roundabout manner at the same approximate result as was obtained by Laplace nearly a century earlier,—a result long

known to be quite imperfect. At the June meeting of the Royal Astronomical Society, Prof. Adams communicated a further paper containing a proof in an elementary form of the accuracy of his own value, impugned as incorrect by Sir George Airy. This paper was published in the middle of August. Although some rejoinder may be looked for from Sir George Airy, there can be no doubt, after Prof. Adams' conclusive reply, that the Astronomer Royal has been led into error by the form he has given to his investigations.

A singular point in this controversy may be mentioned. There has always been a desire to reconcile the observed value of the acceleration in the motion of the Moon with that arising from the diminution in the eccentricity of the Earth; but whereas the former seemed to amount to over 12″·2, the latter was only 6″·2. It was this discrepancy which probably led Sir George Airy to take up the subject. But quite lately the researches of Prof. Newcomb have clearly shown that this value of 12″·2 is a great deal too large, and arises from an error in Hansen's Tables of the motion of the Moon. Correcting this error, Prof. Newcomb is able to show that the secular acceleration is probably less than 8″·3. It has been shown, subsequently, by Mr. Neison, that if one of the most discordant of the ancient eclipses be omitted, all the rest are in harmony with the value of the secular acceleration of 7″·2, and are more in harmony with the value 6″·2 than with any value greater than 8″·0. It is not at all improbable, therefore, that before long further improvements in the lunar theory will reconcile the observations with Prof. Adams value of the secular acceleration, and that Sir George Airy's value, instead of reconciling theory and observation, as he supposed, would have thrown them into intolerable discord.

Record of the Progress of Astronomy during the year 1879.—Under this title there has been compiled by Mr. J. L. E. Dreyer, M.A., of the Observatory of Trinity College, Dublin, a very useful account of the principal astronomical events of the past year, and it has been published as a memoir by the Royal Dublin Society in their *Scientific Proceedings*, and reprinted therefrom in an octavo pamphlet of some fifty pages. Mr. Dreyer prefaces his summary as follows:—'In the following pages I shall endeavour to sketch the principal astronomical events of the past year, giving short accounts of the more important or interesting investigations which have been published during this period. For the year 1878, such an account was written by Prof. E. S. Holden, of the United States Naval Observatory, for the *Annual Record of Science and Industry*; and as I have learned from him that this publication has been discontinued, I have thought it might be of some use, both to professional astronomers and to amateurs, if a continuation of his record was kept up, giving summaries of the work done in the various branches of Astronomy, merely intended to draw attention to what has been done in them.' It had at first been my intention to add to this review a bibliographical list of books and memoirs in Astronomy published during 1879, but for various reasons I have left it out. It might be better to let such a list embrace a longer lapse of time than one year; and, besides, the *Bibliographie Générale*, the publication of which has recently been announced from the Brussels Observatory, is to include the year 1880. The present record does not aim, therefore, at any completeness, but only at

giving a brief account of a number of memoirs and papers which have appeared to me to possess more than a passing interest.' Mr. Dreyer's summary will certainly be found of great use to all astronomers; and it is to be trusted that in the interest of Astronomy its compiler will see his way to rendering it an annual publication. There are few things which a scientific man requires more than a compact record of the principal work achieved during the year—a record giving not only a general idea of the contents of a paper or a memoir—the mere title is of far less value—but the publication wherein it appeared. Mr. Dreyer divides his record into the following seventeen heads:—1. Spherical Astronomy; 2. Theory of Instruments; 3. Celestial Mechanics; 4. The Sun; 5. The Moon; 6. The Intra-mercurial Planet Question; 7. Planets and Satellites; 8. Comets; 9. Meteors and Meteorites; 10. Fixed Stars; 11. Annual Parallax: 12. Double Stars; 13. Nebulæ and Clusters; 14. Photometry; 15. History of Astronomy, Bibliography; 16. Observatories; 17. Miscellaneous Notes.

Figure of Mars.—During the last opposition of *Mars* in November 1879, Professor C. A. Young made a numerous series of measures of the diameter of the planet with a parallel wire micrometer, on the $9\frac{1}{2}$-inch Alvan-Clark Equatorial of the Princetown College Observatory. From a careful discussion of these observations, Prof. Young deduces the following value for the apparent equatorial and polar diameter of the planet on November 12, 1879:—

Equatorial diameter of Mars .	=	$20''{\cdot}634$	$\pm\ 0''{\cdot}034$
Polar diameter of Mars . .	=	$20{\cdot}552$	$\pm\ 0{\cdot}043$
Mean diameter of Mars . .	=	$20{\cdot}593$	$\pm\ 0{\cdot}035$

These measures, being made with a parallel-wire micrometer, must be affected by the constant error due to irradiation at the edge of the planet, the irradiation being principally due to the spurious disc into which every point of light is converted by the telescope. Prof. Young points out that if we adopt as the true value of the diameter of the planet *Mars* at the distance unity from the Earth, the value $9''{\cdot}352$ deduced by Hartwig from all the measures prior to 1879, the real value of the diameter of *Mars* on November 12, 1879, must have been $19''{\cdot}128$. The difference $1''{\cdot}465$, between this and the value found by Prof. Young, which was $20''{\cdot}593$, is regarded by him as being due to irradiation. This is larger than that due merely to the spurious telescopic diffraction disc, which would amount to about $1''{\cdot}0$. Prof. Young points out that the value found by him for the mean diameter of *Mars*, when reduced to distance unity, is $10''{\cdot}068$.

Prof. Young then utilizes these measures to determine the figure of the planet. The difference between the apparent polar and equatorial diameter is $0''{\cdot}0818$, which would correspond to a compression or ellipticity of $1 \div 234$. But as Prof. Young points out, at the time of measurement the pole of *Mars* was not really on the periphery of the planet, but was $14^\circ{\cdot}5$ from it, so that the apparent polar diameter was greater than the real polar diameter. Allowing for this, Prof. Young deduces $1 \div 219$ as the actual polar compression of the planet *Mars*. As Prof. Young points out, this is in close accord with the value $1 \div 228$ deduced by Prof. J. C. Adams for the polar com-

pression of *Mars* on the assumption that the internal constitution of the planet follows the same law as that of the Earth.

On the Brightness and Dimensions of the Satellites to the Planets.—For some time Professor Edward C. Pickering, of the Harvard College Observatory, has been engaged in a series of important photometric investigations; and in Part 2 of Vol. XI. of the *Annals of the Harvard College Observatory*, he has published the results of the observation of the brightness and size of the satellites of the planets.

Mars.—The inner satellite of *Mars*, *Phobos*, is fully 0·3 magnitude brighter than the outer satellite *Deimos*, and *Deimos* is in turn 14·5 magnitudes fainter than the brightness of the central portion of the disc of *Mars*. *Deimos* seems to be variable in brightness, being fully 0·5 magnitude brighter when on the following side of the planet, than when on the preceding side. Assuming that the satellites reflected as much of the incident light as *Mars* itself, Prof. Pickering deduces 6 and 7 miles as the diameter of *Deimos* and *Phobos*, or less than 0″·05 for their maximum apparent diameter. According to Prof. Pickering, a certain amount of light is not prejudicial to the visibility of these faint objects.

Jupiter.—Observations were made of the brightness of the satellites, by comparing them with the planet itself, and from the amount of light they reflected, their apparent size was calculated on the hypothesis that they reflected the same proportion of the incident light as the planet. The results were—

	I. Satellite.	II. Satellite.	III. Satellite.	IV. Satellite.
Photometric diameter	0·873	0·818	0·035	0·615
Measured diameter .	1·081	0·910	1·537	1·282
Albedo	0·652	0·809	0·455	0·230

Or, in other words, the satellites of *Jupiter* reflected only two-thirds, four-fifths, four-ninths, and one quarter of the same proportion of incident light as the planet itself reflects, while this is known to be only six-tenths of the total light falling on the surface. These results are in complete accord with the known phenomena presented by the satellites.

Saturn.—Observations were made of all the satellites of *Saturn*, with results as follows, namely, that the comparative dimensions of the different satellites were, as given below, on the assumption that all reflected the same proportion of the incident light:

Mimas	202	miles.
Enceladus	370	„
Tethys	570	„
Dione	542	„
Rhea	745	„
Titan	1406	„
Hyperion	193	„
Japetus	486	„

If, therefore, *Titan* be taken as usually shining like an 8½ magnitude star, the brightness of the different satellites may be approximately considered to be—

Mimas	12·0	magnitude	(Argelander)	14½	magnitude	(Herschel.)
Enceladus	11·4	„	„	13	„	„
Tethys	10·5	„	„	11½	„	„
Dione	10·8	„	„	12	„	„
Rhea	9·9	„	„	10½	„	„
Titan	8·5	„	„	9	„	„
Hyperion	13·0	„	„	17	„	„
Japetus	10·8	„	„	12	„	„

Japetus is, however, very variable in brightness, being at one part of its orbit more than three times as bright as at another part. Prof. Pickering points out that if this variation in brightness is due to the unequal brightness of the two hemispheres of the satellite, the satellite must always turn the same face to the planet exactly as in the case of our Moon. *Mimas* and *Enceladus* look fainter than they really are, owing to their proximity to the planet.

Uranus.—The comparative diameters of the two brighter satellites are—

Titania	586 miles.
Oberon	544 „

The two minor satellites were too faint for measurement.

Neptune.—The comparative diameter of the satellite of *Neptune* is—

Satellite . . . 2260 miles.

It is thus one of the largest of the secondary members of our solar system.

The suspected Ultra-Neptunian Planet.—No results have been made known with respect to the distant planet believed by Prof. George Forbes to be at present close to the star *β Virginis*. It is probable, therefore, that no attempt has been made to search for this very problematical planet. It will be remembered that Prof. Forbes founded his belief in its existence on a study of the orbits of the different comets of long period, and that he assigned a distance from the Sun of over 100 times that of the Earth, and a period of over 1000 years. Prof. Forbes also pointed out, that in 1857 this supposed planet would be in the position of the star No. 894 in the Greenwich First Seven-Year Catalogue, a star which was only seen in the year 1857, and on no subsequent occasion. We now learn that this star, No. 894, was a tenth-magnitude star, observed by mistake for one of the minor planets, and that it still remains in its place. The hypothesis of Prof. Forbes that this might be his planet therefore falls to the ground. As before pointed out, if Prof. Forbes's planet really existed, it would probably be so faint (like a fourteenth-magnitude star), and would move so slowly that it could not be detected without enormous labour with an exceeding powerful telescope.

Faye's Comet.—This comet was detected by Mr. A. A. Common with the 37-inch reflector of the Ealing Observatory, on the night of August 2nd, very near to the position assigned to it in Dr. Axel Möller's Ephemeris. The comet was very small and faint, so that, although it does not pass its perihelion passage until January 22nd, 1881, it is probable that it will not be seen with any except the few very powerful instruments which are now in existence.

Discovery of Two New Planetary Nebulæ.—In a communication to *Nature* (August 5th, 1880), Prof. Edward C. Pickering, of the Harvard College Observatory, announces the discovery of two very small planetary nebulæ. The one discovered on July 13th, is in R.A. $18^h\ 25^m{\cdot}2$, and Decl. $-25° 13'$; and the second, discovered on July 14th, is in R.A. $18^h\ 4^m{\cdot}3$, and Decl. $-28° 12'$. Both are very minute, and can only be distinguished from stars of about the tenth magnitude by their spectra. Prof. Pickering points out that all attempts which have hitherto been made to determine the parallax of a planetary nebula have been foiled by the uncertainty in determining the exact centre of these bodies, or by the haziness of their borders, but the minuteness of the disc presented by the two newly observed nebulæ would permit of their places being determined with the same accuracy as a star.

BOTANY.

Classification of Cryptogamia.—At the recent meeting of the British Association, Mr. A. W. Bennett laid before the Biological Section a proposed modified classification of the Cryptogamia. He remarked that in Sachs' most recent classification, the Thallophyta (including the Characeæ) are divided into four classes, namely, Protophyta, Zygosporeæ, Oosporeæ, and Carposporeæ. In his proposed classification, Mr. Bennett removes the Characeæ altogether from the Thallophytes as a distinct group. The Thallophyta are then divided into three primary classes, namely, Protophyta, Fungi, and Algæ. The Protophyta are divisible into two sub-classes, *Protomycetes* and *Protophyceæ;* the former including only a single order, the *Schizomycetes;* the latter consisting of the Protococcaceæ (including Palmellaceæ and Scytonemeæ), Nostocaceæ, Oscillatorieæ and Rivularieæ. The *Myxomycetes* are treated as a supplement to the Protophyta.

The Fungi include three sub-classes: the *Zygomycetes* composed of the Mucorini alone; the *Oomycetes,* containing the Peronosporeæ and Saprolegnieæ; and the *Carpomycetes,* comprising Uredineæ, Ustilagineæ, Basidiomycetes, and Ascomycetes, the Lichens being included in the last as a sub-order.

The Algæ also form three sub-classes corresponding with those of the Fungi, namely, the *Zygophyceæ,* including the orders Pandorineæ, Hydrodictyeæ, Confervaceæ, Ulotrichaceæ, Ulvaceæ, Botrydieæ, and Conjugatæ (under the last of which we have the Desmids, Diatoms, Zygnemaceæ, and Mesocarpeæ); the *Oophyceæ,* comprising Volvocineæ, Siphoneæ, Sphærophyceæ, Œdogoniaceæ, Fucaceæ, and Phœosporeæ; and the *Carpophyceæ,* including only the Coleochætеæ and Florideæ.

The Characeæ, as already stated, are regarded as forming a distinct primary group. The Muscineæ are retained in the same sense as by Sachs. In the case of the Vascular Cryptogamia, Mr. Bennett proposes to adopt the primary distinction into Isosporia and Heterosporia as most in accordance with probable genetic affinities. The former include the Filices, Lycopodiaceæ and Equisetaceæ; the latter the Rhizocarpeæ and Selaginellaceæ.—*Nature, 9th September.*

CHEMISTRY.

Formation of Ozone during the Evaporation of different Liquids.—When it is desirable to show a number of persons that ozone is formed during the evaporation of a liquid, Böttger (*Polyt. Notizblatt*, xxxv. 95) recommends letting fall on a piece of paper which has been uniformly saturated with a solution of starch and iodide of cadmium some drops of alcohol or ether, and the igniting of such solution. The paper in consequence of the ozone which is formed by the evaporation acquires a dark blue colour.

Compound of Phosphoric Hydride and Chloric Hydride.—This compound is easily prepared according to Lemoine (*Bull. Soc. Chim. Paris*, xxxiii. 194), by placing the two gases in a U-tube, one end of which is closed, under a pressure of two atmospheres, produced by a column of mercury in an open tube and exposing the apparatus to the cold of liquid sulphurous acid, the evaporation of which is increased by a strong current of air.

Silver Trioxide.—By the electrolysis of silver nitrate a curious body is formed, first described by Ritter in 1804, and later by Grothus, which was first regarded as silver dioxide, but has since been shown to contain nitric acid. Berthelot has since examined it (*Compt. Rendus*, 1880, xc. 653). He employed a battery of four Bunsen elements during twenty-four hours, and obtained a gramme of the body. Washed and dried it presented the appearance of thick, short, flattened needles with metallic lustre, which soon fall to pieces and lose their lustre. It developes oxygen rapidly at 100°. Analysis gave the formula $4\,Ag\,O_3$, $N\,O_6\,Ag$, $H\,O$ (old formula!). The longer it lies the higher rises the percentage of silver, and the composition approaches the formula $N\,O_6\,Ag + 4\,Ag\,O$.

Salireton.—Experiments have been made by P. Giacosa (*Journ. prakt. Chem.* 1880, xxi. 221), to prepare glycosides synthetically, and led him to heat saligenin and mannite to 100°. In this way he obtained a new product of condensation of saligenin $C_{14}\,H_{12}\,O_3$, which he has termed salireton. The same body is formed when glycerin or methylal is taken in place of mannite: and its formation is thus explained:

$$2\,C_7\,H_8\,O_2 - H_2\,O - H = C_{14}\,H_{12}\,O_3.$$

The curious phenomenon that during the formation of this body, not only water but hydrogen also should be extruded, led the author to think that perhaps, in the first instance, a molecule of saligenin might be oxidized to salicylaldehyde, which then first combined with a second molecule of saligenin to salireton, water being extruded, as shown below:

$$C_7\,H_8\,O_2 + C_7\,H_6\,O_2 = C_{14}\,H_{12}\,O_3 + H_2\,O.$$

Although when saligenin was heated with glycerin the odour of salicylaldehyde could not be recognized, the author heated equivalent quantities of salicylaldehyde and saligenin, just like the saligenin and glycerin, in closed tubes in boiling water. After many hours' heating, the contents of the tubes were treated with water and distilled; with the steam almost the whole of the salicylaldehyde passed over; the resinous residue remaining in the retort was boiled with a little water and filtered. From this filtrate the salireton separates: this is recrystallized, and when pure melts at 121°·5. The yield

of salireton was far less than when saligenin is heated with glycerin or methylal. In any case the salicylaldehyde, like the glycerin, plays the part only of a liquid of suspension. The salireton melts at 121°·5, gives off gas at 140° as well as the odour of salicylaldehyde, while a black resinous residue remains. An examination of this residue showed it not impossible that this contained saliretin $C_7 H_8 O$ (?). For the formation of the latter body the following reaction may perhaps take place:

$$\underset{\text{Salireton.}}{C_{14} H_{12} O_3} - \underset{\text{Salicylaldehyde.}}{C_7 H_6 O_2} = \underset{\text{Saliretin.}}{C_7 H_6 O}$$

Recognition of Mercury.—Jüptner employs the iodide $Hg I_2$, which, as is well known, is dimorphous, occurring in yellow rhombic crystals as well as scarlet square octohedra (*Oest. Zeitung*, 1880, xxviii. 92). The latter modification, in which mercury is obtained in the wet way, as well as by touching the yellow body produced by sublimation, possesses the property of appearing quite white in the light of the sodium flame. The author studied the appearance of various colours under such conditions, and examined a series of yellow, red, green, and blue colours by the light of sodium and lithium, and found that none of them were affected in the same manner as by mercury biniodide. It forms a means by which mercury can be recognized with certainty.

Production of Alcohol by an Electric Current.—Berthelot's experiment was recently conducted in the following manner: A battery of eight Bunsen elements was connected with an oscillating conmmutator, so that from twelve to fifteen currents were sent in alternate directions per second, which were conducted to two electrodes of spongy platinum. The platinum cylinder was placed in acidulated water, and the contact-action so arranged that neither oxygen nor hydrogen were given off, but the decomposed water regenerated as rapidly as it had been split up. When all had thus been arranged, the two electrodes of the apparatus were immersed in a watery solution of glucose. In this way alcohol was obtained, although it is true in but small quantity. It is hoped, however, by modifying the apparatus that the process may be improved and even made of technical importance.—(*Chem. Central Blatt.*, 1880, 288.)

Remarkable Behaviour of Tin Chloride and Potassium Chlorate.—According to Böttger (*Polytechn. Notizblatt*, xxxv. 96), when two parts by weight of tin chloride and one of potassium chlorate are rubbed together (they must previously have been rubbed fine in a porcelain mortar) the mass becomes strongly heated in a few seconds; chlorous acid and a large quantity of vapour of water are given off. The residue is a yellowish-white mass which dissolves in boiling water, and, on cooling, gives splendid brilliant mica-like crystals of potassium perchlorate, the supernatant opalescent, milk-like liquid containing the oxychloride in solution.

The Tannic Acid of Oak Bark.—C. Etti prepared the acid from the product of extracting oak-bark with alcohol by digesting it with acetic ether, in which it is soluble. He obtained a reddish-white amorphous powder, the analysis of which led to the formula $C_{17} H_{16} O_9$ as that of the body. From it, by heating it to 140°, or by boiling its aqueous solution with dilute acids, three anhydrides were obtained. The first anhydride is $C_{34} H_{30} O_{17}$, the second is $C_{34} H_{28} O_{16}$, and the third anhydride $C_{34} H_{26} O_{15}$: the first is identical with the natural phlobaphen of oak-bark, the third with Oser's 'oak-

red.' From the tannic acid, by heating it with acids in closed tubes, only gallic acid as well as 'oak-red' were obtained. If this operation be performed with hydrogen chloride, the development of a gas, which burns with a green flame (methyl chloride), is observed. The dry distillation of tannic acid yielded a small amount of brenzcatechin and a mixture of oily products, in which, very probably, the presence of dimethylbrenzcatechin was remarked. When fused with potassium hydrate there are formed, as Grabowski has shown, protocatechic acid, brenzcatechin, and phloroglucin. Digested with emulsion, or boiled with dilute acid, it yields no sugar-like bodies. It cannot, however, be regarded as glycoside—a conclusion which will already have been arrived at, from the numbers indicating an anhydride. (*Wien Anz.* 1880, 61.)

The Passivity of Iron.—If a bright piece of iron be dipped into nitric acid of 42° B, a brisk effervescence of gas takes place; this soon suddenly stops, and the metal appears bright and lustrous, and remains so. This occurs also when more dilute acid is used, down to 35° B. With the latter, also, passivity sets in after a certain time, but it does not last, and after a certain time the action begins again at a point of the surface, to suddenly stop again later on. If an iron rod be partly dipped into strong acid between 42° and 35° B, and then it be brought, with very great care, into more dilute acid, the already moistened part first, and then the upper part of the rod is slowly introduced, no action takes place at first; after some time some bubbles are seen to form on the metal, which soon suddenly cease to be developed, the rod becomes passive, and has become so because the rod had been dipped partly into strong acid. This condition has, however, no great stability; a slight shaking is sufficient to bring about a change. The degree of passivity produced in this manner is variable, and, in fact, is the smaller, the more dilute the second acid, the rougher the surface of the metal, and the greater the diameter of the rod. As soon as the metal is brought into a concentrated acid chemical action takes place. The bubbles of gas generated by the action are at first readily dislodged from the metal and are taken up by the surrounding liquid. This solubility, however, is limited, and the bubbles of gas remain adhering by capillary attraction to the metal, forming a protecting coat, which, however, can be dislodged by shaking. If the acid, however, is less concentrated—say 36° B.—the phenomenon is intermittent, because the coating is dissolved after some time, and a new one will then be formed. In the experiments with the partly immersed iron, the action of the adhering film of gas is propagated by the slow immersion of the iron slowly upwards, and exercises an attraction on the newly-formed bubbles, whereby the coating is continued from one part to another. In order to determine the influence of the dilution of the acid on the duration of the passivity, the author, L. Varenne (*Compt. Rendus*, 1880, xc. 998), made specimens of iron passive, and introduced them into acids of different degrees of dilution, as below:—

In an acid of	34° B,	the passivity of	iron ceased	after 11	days.
„	32° B,	„	„	5	„
„	30° B,	„	„	32	hours.
„	28° B,	„	„	26	„
„	25° B,	„	„	24	„
„	20° B,	„	„	12	„

When the experiments were conducted *in vacuo* the same rule held good, the passivity ceasing a little earlier.

GEOLOGY.

Fossil Vertebrates from the Eocene of Champagne.—Dr. Victor Lemoine has devoted much attention to the remains of vertebrate animals found in the Eocene deposits of the neighbourhood of Rheims, from which he states he has obtained indications of over a hundred new species. Of these about forty are Mammalia, representing the Carnivorous, Insectivorous, Rodent, and Pachydermatous types. Of Birds, he has five species, some of them large, and presenting certain characters which approximate them to Reptiles; whilst, on the other hand, certain of the Reptilian bones present more or less avian characters. The Fishes, also, are said to offer a remarkable approach to the Reptiles. In the case of the Mammalia, he remarks that their predominant peculiarity is that they present mixed types, and the complexity of this mixture is greater in proportion to the antiquity of the animal. His Carnivora are complex types, having resemblances with Pachyderms, Lemurians, and Marsupials. The dentition in *Arctocyon*, of which Dr. Lemoine has two new species, presents a sort of combination of that of the Ursidæ and that of the Porcidæ, especially *Entelodon;* whilst the form of the cranium, the inclination of the angle of the lower jaw and the perforation of the humerus seem to be Marsupial characters, and the caudal vertebræ are somewhat analogous to those of the Lemurs. In the strictly carnivorous group, M. Lemoine places a new genus, *Hyænodictis;* whilst a *Proviverra* represents the less carnassial Canidæ and Viverridæ. Some small mammals, apparently organized for climbing, may have resembled the lemurs of Madagascar, as would appear from the dentition of some of them, which, however, is varied so that while certain species appear to have been insectivorous, others were probably frugivorous, and others again fitted for a mixed diet. Of these forms, which he is inclined to class under Cope's term, Mesodontes, Dr. Lemoine notices numerous species, which he refers to the genera *Protoadapis*, Lem.; *Plesiadapis*, Gerv., and *Miacis*, *Diacodon*, and *Opisthotomus* of Cope. A single molar is said to resemble the same tooth in *Phenacodus*, Cope. If these determinations be confirmed, they will indicate an interesting analogy between the Eocene faunas of France and New Mexico, and a similar analogy in the floras has already been noticed by Saporta.

Two other forms appear to belong to the American group of the Tæniodontes, and present resemblances to the Mesodontes just mentioned, and to existing Rodents and Edentates. Similar analogies seem to prevail in the types M. Lemoine regards as representing the Pachyderms. The even-toed Pachyderms are represented by two species of *Dichobune*, and others, forming a new genus, *Lophiodochærus*. The Perissodactyla are more numerous, and include species of *Hyracotherium*, *Lophiodon*, and *Coryphodon*, with other forms which present very singular and varied resemblances. A molar of *Halitherium* was also obtained from the *Calcaire grossier* of Rheims.

The remains of Birds all indicate Palmipede, or Grallatorial forms.

Among them is a new species of *Gastornis* (*G. Edwardsii*). Among Reptiles, freshwater and marsh tortoises abound, belonging to the genera *Emys, Dermatemys, and Platemys*, the last now confined to the Southern hemisphere. Several species of *Trionyx* occur, and also a new type of Tortoise, resembling *Trionyx* in the granular state of its carapace, and the Emydes in the marginal plates, and the structure of the plastron. Both true Crocodiles and Alligators have left their remains in these deposits, and some of them appear to have been of large size. Among the Crocodiles one seems to approach the Gavial in the structure of the lower jaw; whilst another presents analogies with certain Secondary Crocodiles in its dental structure. Of the Lacertilian forms some have concavo-convex vertebræ; whilst others, with the vertebræ biconcave, or flat on both surfaces, would seem to belong to the Geckotidæ, a group now including only the Geckos, but more largely represented in Secondary formations. A new genus *Simædosaurus*, seems to present a combination of Lacertian and Crocodilian character, and was probably aquatic in its habits. Remains of snakes indicate that those animals may have attained a length of ten or twelve feet. The Batrachia are represented by the genus *Bufo*. The remains of Teleostian, Ganoid, and Chondropterygian fishes have been detected. Among the Ganoids the author mentions *Phyllodus, Lepidosteus*, and *Amia;* and of the cartilaginous fish he cites Chimæras, Sharks, and Rays. Remains of Mollusca, fragments of Insects, Entomostraca, and Foraminifera, together with many traces of plants, also occur in the Lower Eocene of the neighbourhood of Rheims.—(*Compte Rendus de l'Assoc. Franç.*, 1879, p. 585.)

British Fossil Cephalopoda.—We understand that the Rev. J. F. Blake, F.G.S., assisted by grants from the Government Research Fund administered by the Council of the Royal Society, has now brought the first stage of his investigations upon the British Fossil Cephalopoda to a close. He has completed his work upon the Silurian forms, which he proposes shortly to publish, prefixing to his descriptions of the genera and species a general introduction on the anatomy of the group, the structure and modification of the shells, &c. The number of Silurian species recognized by him is nearly two hundred. The work will form a volume of about four hundred 4to pages, illustrated with thirty-two plates. It will be published by subscription.

Mesozoic Mammalia.—At the close of his description of some very interesting Jurassic Mammalian remains from the Rocky Mountains (*American Journal*, September 1880), Prof. Marsh adds the following general remarks, which may be taken into consideration side by side with the facts detailed in Dr. Lemoine's account of the Mammalia of the Lower Eocene of Rheims:—

'Mesozoic Mammals have been very generally referred hitherto to the *Marsupialia*. An examination of all the known remains of American Mesozoic Mammalia, now representing upwards of sixty distinct individuals, has convinced the writer that they cannot be satisfactorily placed in any of the present orders. This appears to be equally true of the European forms which the writer has had the opportunity of examining. With a few possible exceptions, the Mesozoic Mammals best preserved are manifestly low generalized forms, without any distinctive Marsupial characters. Not a few of

them show features that point more directly to Insectivores, and present evidences, based on specimens alone, would transfer them to the latter group, if they are to be retained in any modern order. This, however, has not yet been systematically attempted, and the known facts are against it.

'In view of this uncertainty, it seems more in accordance with the present state of science to recognize the importance of the generalized characters of these early Mammals, as at least of ordinal value, rather than attempt to measure them by specialized features of modern types, with which they have little real affinity. With the exception of a very few aberrant forms, the known Mesozoic Mammals may be placed in a single order, which may appropriately be named *Pantotheria*. Some of the more important characters of this group would be as follows:—

(1) Cerebral hemispheres smooth.
(2) Teeth exceeding, or equalling, the normal number, 44.
(3) Premolars and molars imperfectly differentiated.
(4) Canine teeth with bifid or grooved fangs.
(5) Rami of lower jaw unankylosed at symphysis.
(6) Mylohyoid groove distinct on inside of lower jaw.
(7) Angle of lower jaw without distinct inflection.
(8) Condyle of lower jaw near or below horizon of teeth.
(9) Condyle vertical or round, not transverse.

'The generalized numbers of this order were doubtless the forms from which the modern specialized Insectivores and Marsupials, at least, were derived.

'Another order of Mesozoic Mammals is evidently represented by *Plagiaulax*, the allied genus *Ctenacodon*,* and possibly one or two other genera. These are all highly specialized aberrant forms, which apparently have left no descendants. This order, which may be termed *Allotheria*, can be distinguished from the previous group by the following characters:—

(1) Teeth much below the normal number.
(2) Canine teeth wanting.
(3) Premolar and molar teeth specialized.
(4) Angle of lower jaw distinctly inflected.
(5) Mylohyoid groove wanting.

'These characters alone do not indeed separate the Plagiaulacidæ from some of the Marsupials, and future discoveries may prove them to belong in that group, where they would then represent a well-marked sub-order.'

MINERALOGY.

Crystallized Danburite.—In the *American Journal of Science* for August, 1880, there is a paper by Professors Brush and Dana on the occurrence of this mineral at Russell, St. Lawrence County, New York. They received a number of specimens from a mineral collector in Northern New York, and amongst them were some prismatic white weathered crystals, which bore

* A fine jaw of which is figured by Prof. Marsh in this paper.

no name. An examination with the blow-pipe showed it to be an anhydrous boro-silicate, corresponding in physical characters with the rare species danburite. Further specimens were obtained in the spring of the present year, and these were found on further examination to establish beyond all question the identity of this mineral with danburite. It occurs both crystallized and massive imbedded in a granitic rock; the points at which it is found extend along the brow of a hill for a considerable distance, say half a mile. The crystals line cavities or seams, sometimes of very considerable size, in the massive mineral or the enclosing rock. The associated minerals are a pale-green pyroxene, a dark-brown tourmaline, and some mica, quartz and pyrites. The danburite often encloses the crystals of pyroxene and tourmaline, and is itself imbedded in the quartz, which is a point of interest in connection with its time of formation. These cavities were, doubtless, all filled originally with calcite, as the facts observed conclusively prove. A few perfectly fresh specimens were found with the crystals, imbedded in pink calcite. The perfectly pure, clear, and transparent crystals found in the calcite are of rare beauty. Most of the specimens are now nearly, or quite, free from calcite, that mineral having evidently been removed by slow solution. The crystals are thus left in their original position, projecting into the cavities. This natural removal of the calcite is in some aspects of the case an advantage, and in others quite the reverse. In no other way could the crystals have been freed from the calcite so perfectly and with so little injury to themselves; for mechanical removal is out of the question, owing to the brittleness of the mineral; and the removal by chemical means in the laboratory would not leave the crystals so nearly in their original condition. The danburite, as has been stated, is in part crystallized, in part massive. The crystals vary from those which are very minute to others which are of considerable size. The largest isolated crystals have a length of four, and a width of two and a half inches; some of the groups are really grand in their proportions. The massive mineral can be obtained in large blocks; it shows brilliant lustre, is quite unaltered, and almost free from admixed species. The most striking point in regard to the crystals is their similarity to crystals of topaz; so close is this resemblance that the specimens, if not examined too critically, might be handled many times without a suspicion that they did not belong to that species. There is, in fact, a true homœmorphic relation between the two species. The cleavage is basal, as in topaz, but not very distinct. The hardness is 7 to 7·25, and the specific gravity 2·986 to 3·021. The lustre of the polished crystalline surface is very brilliant; on the fracture and in the massive mineral it is vitreous to greasy : in this form it has much the aspect of common varieties of quartz. The colour in the freshest crystals imbedded in calcite is pale wine-yellow, in others pure yellowish-white to honey-yellow, dark wine yellow and yellowish brown. The crystals belong to the orthorhombic system; they are uniformly prismatic in habit, and it is not hemimorphic. The axial ratios are found to be, for—

	c (vert.)	b	$\breve{a}$
Danburite . . .	0·8830	1·8367	1·0000
Topaz . . .	0·9024	1·8920	1·0000

The above values show that the two species are closely homœmorphous. The mean index of refraction (β) for danburite is found to be—

$$\beta = 1{\cdot}634. \text{ Red (Li)}$$
$$= 1{\cdot}637. \text{ Yellow (Na)}$$
$$= 1{\cdot}646. \text{ Blue } (Cu\,SO_4)$$

In the case of the two minerals the mean indices of refraction are not far apart; thus for the D line in the spectrum we have—

$$\beta \text{ Danburite} = 1{\cdot}637$$
$$\text{Topaz} = 1{\cdot}6138$$

The means of several analyses showed the mineral to have the composition given below:—

Silicic acid . . .	48·23
Boracic acid . . .	26·93
Lime	23·24
Alumina	0·47
Lost by ignition . .	0·63
	99·50

These numbers agree closely with those found by Smith and Brush when examining the Danbury mineral, and indicate the formula $Ca_2\,Si\,O_4 + B_4\,Si_3\,O_{12}$ as that of the mineral. There does not appear to be any immediate relation between danburite and topaz in chemical composition, which, considering the similarity in crystalline form, is rather remarkable. The mineral is slightly acted upon by hydrochloric acid, sufficiently to give the reaction of boracic acid with turmeric paper. When previously ignited to the point of fusion the mineral gelatinizes with acid. The crystallographic results obtained in the case of the Russell specimens do not accord with those found with the specimens from Danbury, Conn.; and this is accounted for by the crystalline specimens in the latter locality being imbedded in felspar, 'where apparent planes, at best of a problematical nature, certainly did not represent the crystalline form of the species.'

The Copper present in Coal.—An examination by Stolba of specimens of coal chiefly from Bohemia shows the invariable presence of a small quantity of copper. (*Sitzber. böhm. Gesellschaft der Wiss.*, April, 1880.) The ashes of the coals, carefully prepared, always showed a strong copper reaction; the pure coal itself contained a trace only of copper; the pyrites accompanying it gave a strong reaction. In fact the strength of the reaction appears to go hand in hand with the amount of pyrites present in the coal. The never-failing copper of the coal determines the amount of copper present in iron prepared with such coal or coke. The copper present in the coal with which we heat our ovens can be shown by the following simple method. When the coal is burnt and ceases to give a flame, and only the so-called glow is to be observed, a spoonful of pure salt is to be thrown upon it, and stirred about with a tongs or stick of wood. Immediately the azure-blue flames of carbonic oxide containing copper chloride are produced, and the appearance lasts some time. Coal which contains much pyrites exhibits the colour with great intensity and in great beauty. This, doubtless, is the cause of the colour which is so familiar to most English people, and for which many explanations have been proposed.

PHYSICS.

A Vacuum Tube of variable Resistance was exhibited before the Physical Society by Dr. Stone, at the last meeting for the season, on June 26th. It consisted of a barometer tube 32 inches long, terminating above in a short vacuum chamber arranged transversely, and closed at either end by adjustible india-rubber stoppers, through which platinum terminals passed. Above this the vertical tube continues to a glass stopcock, by means of which small quantities of air can be introduced. The foot of the tube is attached to a flexible india-rubber pipe, with a cistern similar to that of Frankland's gas apparatus. The cistern full of mercury is counterbalanced, and can be raised or lowered through the whole 32 inches. A Torricellian vacuum can thus be made in the upper chamber, or one of less perfectness. On passing the induction spark between the terminals in the former case, all the discharge is carried off, none appearing at the discharger. By gradually raising and lowering the cistern, after admitting a little air by the stopcock, the resistance of the partial vacuum can be altered within wide limits. A point can be found where the spark of breaking contact is shunted through the vacuum tube, while the weaker discharge of making contact is stopped. The induction-current is thus obtained in a single direction, a matter of some importance in physiological experiments.

Pneumatic Clocks have been successfully established in Paris, both for public and private purposes. The subscribers are supplied with dials on this system for the sum of a halfpenny per day. Air is compressed to five atmospheres in a reservoir at the central station. A distributing-clock places this in communication with distributing-pipes for twenty seconds every minute, the used air being again employed to wind automatically the original train. The distributing-tubes are of iron, 27 millim. in bore, carried underground. These, by leaden or indiarubber connexions, communicate with the affiliated dials. The dial has a small caoutchouc bellows, similar to that of the pneumatic telegraph, acting on a lever, which takes, by means of a ratchet, into a wheel of 60 teeth, carrying the minute-hand. The hour-hand is moved by the usual motion-work. Striking-clocks are also fitted up on the same system for the small increase in price of a single centime, namely, six instead of five per diem. It appears that the whole expense is from fifteen shillings to a pound per annum.

The Magneto-Optic properties of Gases are being investigated by M. Henri Becquerel. He has recently examined oxygen, hydrogen, carbonic dioxyde, nitrous oxyde, and olefiant gas. Except in the case of oxygen, the magnetic rotation of the plane of polarization due to a field of given intensity varies inversely as the square of the wave-length of the ray, as is the case in solids and liquids. This implies that violet rays are more rotated than red, or that there is a positive dispersion. In the case of oxygen, it is found that the red rays are rotated more than the green. This is the more remarkable, as oxygen gives a positive rotation as if it were a diamagnetic body. He remarks that oxygen behaves as if it were a mixture of a magnetic and a diamagnetic body, the magnetic having small negative rotation and a great negative dispersion, the diamagnetic having great rotation and small positive dispersion.

A New form of Electro Magnet was exhibited to the Physical Society by Dr. Stone. It was wound with best charcoal annealed iron wire of 5 millim. section, in four parallel circuits. Each pole was cast after winding into a solid block of paraffin, and turned in a lathe. It was expected that the latter device would increase the inductive effect of the spirals, and it appeared that the lifting power was somewhat augmented. The cores had originally been wound with large copper-wire, of the same weight, in three parallel circuits. The lifting power for moderate batteries, of from five to six Bunsen's cells, had been increased fourfold by the substitution of the iron. The object was to produce a large diffused magnetic field for the purpose of physiological experiments on anæsthesia and other nervous diseases, in which there seemed to be ground for the belief that the effects of magnetism were far from inappreciable.

The Resolving Power of Telescopes, in its experimental point of view, is examined by Lord Rayleigh. He states that the only work on the subject he is acquainted with is that of Foucault, who examined the resolving power of a telescope of 10 centim. aperture on a distant scale lighted by direct sunshine.

The object viewed in these experiments was a grating of fine wires formed by screwing the ends of a stout brass wire, bending it into a horse-shoe, and winding wire of half the pitch diameter into the grooves of the screw. The whole is fixed with solder, and the wires on one side are cut away. For rough purposes, common wire gauze of 30 to 40 meshes per inch answers. The grating was backed by a soda flame, though a common paraffin lamp may be substituted.

The telescope was provided with a cap to which various diaphragms can be fitted over the object-glass. These may be circular or rectangular, the latter being placed with its long axis parallel to the wires of the grating.

The observation consisted in finding the greatest distance at which the wires can be seen apart. This proved to be more definite than might have been expected, not differing more than 2 or 3 per cent for various observers. Two slits, half an inch long, and of ·107 and ·196 inch wide, were used. The width was taken by a wedge and measured by callipers. The distances were 91·5 and 168·5 inches. These corresponded to angular intervals between consecutive lines of $\frac{1}{4575}$ and $\frac{1}{8425}$. According to theory, the minimum angle is about that subtended by the wave-length of light, λ, at a distance equal to the width of the slit a. In this case $\lambda = 5{\cdot}89 \times 10^{-5}$ centim. and $a = {\cdot}107 \times 2{\cdot}54$ or $196 \times 2{\cdot}54$ centim. Thus: $\frac{\lambda}{a} = \frac{1}{4615}$ or $\frac{1}{8475}$, agreeing closely with the observations. Circular apertures were also used with less satisfactory results. To have equal resolving power, the circular aperture must be about a tenth part wider than the slit.

To show the dependence of resolving power on aperture, it is sufficient to look at wire-gauze backed by the sky or by a flame, through a piece of cardboard pierced by a needle and held close to the eye. By varying the distance a point is easily found at which resolution ceases; but the telescope allows the use of a wider, and therefore more easily measurable aperture.

A new Instrument for the Detection and Measurement of an Inflammable Gas in Mines was brought before the Physical Society by Mr. E. H. Liveing. He noticed that instruments already existing for this object were either physical or chemical. Of the former category were Ansell's and Forbes's: one depending on diffusion; the other on sound-velocity. Of the latter were the flame-test, Coquillion's instrument, and the one now first described.

The principle is as follows:—A mixture of marsh-gas and air, in which the former forms less than 5 per cent, is not explosive or capable of continuing its own combustion at ordinary temperatures and pressures, because the heating value of the marsh-gas is insufficient to raise the large excess of atmospheric air to the necessary ignition-temperature. If, however, such a mixture is exposed to some sufficiently heated object, especially platinum, it will burn in its immediate contact and neighbourhood, and, in so doing, add materially to the temperature of the object; the more so the larger the percentage of gas present.

Two small similar spirals of fine platinum-wire are placed in the same circuit from a small magneto-electric machine. One is enclosed in a tube containing air, the other is a cylinder of wire-gauze; both furnished with glass ends, the latter exposed to the gas to be examined. If the air contain above $\frac{1}{100}$ of marsh-gas, the spiral exposed to it increases in brilliancy, and can be made to determine the percentage of gas present. This is accomplished by placing a wedge-shaped screen between the two spirals and viewing it through a side-tube. The screen can be moved towards either end till equal illumination is obtained, and an empirical scale can then be read off, giving the percentage of gas.

Certain Effects of Stress on Soft Iron Wires have been studied by Mr. J. Ewing, at Tokio, Japan. The wire was Japanese, and annealed. It was hung vertically from a strong frame, and a tank holding 100 kilos. of water was attached to its lower end. The weight of the tank was balanced. It was circular, and of uniform diameter, the stress thus being proportionate to the height of water, as recorded horizontally on a sheet of paper drawn along by a float. A pencil traversed the paper at right angles through distances proportional to the wire's elongation. A continuous diagram was thus drawn automatically from zero to breaking point.

When a constant stress was maintained for a considerable time ($45\frac{1}{2}$ hours), the effect was remarkable. The wire, instead of continuing to lengthen at once when the flow of water was resumed, refused to stretch further until the stress rose to 40 from 35 kilos. There was in fact a new 'limit of elasticity' at this point. It broke with 41 kilos. This 'hardening effect' depended on the length of time during which the trial is interrupted, increasing more rapidly at first than after. If the load were entirely removed, no considerable lengthening took place till a far higher stress than before was reached. Thus an interval of no stress had a hardening effect, like one of constant stress. A stress, therefore, produces two effects, (1), a gradual viscous elongation, at first rapid, afterwards slow; (2), a hardening effect, also greatest at first. The effects are also perceptible in copper and brass, but much less than in soft iron.

Selenium and the Photophone forms the subject of a recent communication, from Mr. Graham Bell and Mr. Sumner Tainter, to the American Association. If the facts therein recorded bear the test of further examination, a very important and startling addition has been made to our knowledge of the possible relations between sound and light. The discovery is best given in the author's own words, as reported by our weekly contemporary, *Engineering.*

'The final result of our researches,' say the authors, 'has evidenced the class of substances sensitive to light vibrations, until we can propound the fact of such sensitiveness being a general property of all matter. We have found this property in gold, silver, platinum, iron, steel, brass, copper, zinc, lead, antimony, German silver, Jenkin's metal, Babbitt's metal, ivory, celluloid, gutta-percha, hard rubber, soft vulcanized rubber, paper, parchment, wood, mica, and silvered glass; and the only substances from which we have not obtained results are carbon and thin microscopic glass. We find that when a vibratory beam of light falls upon these substances they emit sounds—the pitch of which depends upon the frequency of the vibratory change in the light. We find further that, when we control the form or character of the light-vibration on selenium, and probably on the other substances, we control the quality of the sound and obtain all varieties of articulate speech. We can thus, without a conducting wire, as in electric telephony, speak from station to station, wherever we can project a beam of light. We have not had opportunity of testing the limit to which this photophonic influence can be extended, but we have spoken to and from points 213 metres apart; and there seems no reason to doubt that the results will be obtained at whatever distance a beam of light can be flashed from one observatory to another. The necessary privacy of our experiments hitherto has alone prevented any attempts at determining the extreme distance at which this new method of vocal communication will be available.'

They then proceed to describe the peculiar properties of selenium under the influence of light, remarking that 'all observations by previous authors had been made by means of galvanometers; but it occurred to us that the telephone, from its extreme sensitiveness to electrical influences, might be substituted with advantage. Upon consideration of the subject, however, we saw that the experiments could not be conducted in the ordinary way for the following reason: The law of audibility of the telephone is precisely analogous to the law of electric induction. No effect is produced during the passage of a continuous and steady current. It is only at the moment of change from a stronger to a weaker state, or *vice versâ,* that any audible effect is proposed, and the amount of effect is exactly proportional to the amount of variation in the current. It was, therefore, evident that the telephone could only respond to the effect produced in selenium at the moment of change from light to darkness, or *vice versâ,* and that it would be advisable to intermit the light with great rapidity, so as to produce a succession of changes in the conductivity of the selenium, corresponding in frequency to musical vibrations within the limits of the sense of hearing. For we had often noticed that currents of electricity, so feeble as to produce

scarcely any audible effects from a telephone when the circuit was simply opened or closed, caused very perceptible musical sounds when the circuit was rapidly interrupted, and that the higher the pitch of sound the more audible was the effect. We were much struck by the idea of producing sound by the action of light in this way. Upon further consideration it appeared that all the audible effects obtained from variations of electricity could also be produced by variations of light acting upon selenium. We saw that the effect could be produced at the extreme distance at which selenium would respond to the action of a luminous body, but that this distance could be indefinitely increased by the use of a parallel beam of light, so that we could telephone from one place to another without the necessity of a conducting wire between the transmitter and receiver. It was evidently necessary, in order to reduce this idea to practice, to devise an apparatus to be operated by the voice of a speaker, by which variations could be produced in a parallel beam of light, corresponding to the variations in the air produced by the voice.

'We proposed to pass light through a small number of small orifices, which might be of any convenient shape, but were preferable in the form of slits. Two similarly perforated plates were to be employed. One was to be fixed and the other attached to the centre of a diaphragm actuated by the voice, so that the vibration of the diaphragm would cause the movable plate to slide to and fro over the surface of the fixed plate, thus alternately enlarging and contracting the free orifices for the passage of light. In this way the voice of a speaker could control the amount of light passed through the perforated plates without completely obstructing its passage. This apparatus was to be placed in the path of a parallel beam of light, and the undulatory beam emerging from the apparatus could be received at some distant place upon a lens, or other apparatus, by means of which it could be condensed upon a sensitive piece of selenium placed in a local circuit with a telephone and galvanic battery. The variations in the light produced by the voice of the speaker should cause corresponding variations in the electrical resistance of the selenium employed: and the telephone in circuit with it should reproduce audibly the tones and articulations of the speaker's voice. I obtained some selenium for the purpose of producing the apparatus shown; but found that its resistance was almost infinitely greater than that of any telephone that had been constructed, and we were unable to obtain any audible effects by the action of light. We believe, however, that the obstacle could be overcome by devising mechanical arrangements for reducing the resistance of the selenium, and by constructing special telephones for the purpose. We felt so much confidence in this that, in a lecture delivered before the Royal Institute of Great Britain, upon the 17th of May, 1878, was announced the possibility of hearing a shadow by interrupting the action of light upon selenium.

'The first point to which we devoted our attention was the reduction of the resistance of crystalline selenium within manageable limits. The resistance of selenium cells employed by former experimenters was measured in millions of ohms, and we do not know of any record of a selenium cell measuring less than 250,000 ohms in the dark. We have succeeded in producing sensitive selenium cells measuring only 300 ohms in the dark, and 155

ohms in the light. All former experimenters seemed to have used platinum for the conducting part of their selenium cells, excepting Werner Siemens, who found that iron and copper might be employed. We have also discovered that brass, although chemically acted upon by selenium, forms an excellent and convenient material; indeed we are inclined to believe that the chemical action between the brass and selenium has contributed to the low resistance of our cells by forming an intimate bond of union between the selenium and the brass. We have observed that melted selenium behaves to the other substances as water to a greasy surface, and we are inclined to think that when selenium is used in connexion with metals not chemically acted upon by it, the points of contact between selenium and the metal offer a considerable amount of resistance to the passage of a galvanic current. By using brass we have been enabled to construct a large number of selenium cells of different forms. The mode of applying the selenium is as follows: The cell is heated, and, when hot enough, a stick of selenium is rubbed over the surface. In order to acquire conductivity and sensitiveness, the selenium must next undergo a process of annealing.

'We simply heat the selenium over a gas stove and observe its appearance. When the selenium attains a certain temperature, the beautiful reflecting surface becomes dimmed. A cloudiness gradually extends over it, somewhat like the film of moisture produced by breathing upon a mirror. This appearance gradually increases, and the whole surface is soon seen to be in the metallic, granular, or crystalline condition. The cell may then be taken off the stove, and cooled in any suitable way.

'We have devised about fifty forms of apparatus for varying a beam of light in the manner required, but only a few typical varieties need be shown. The source of light may be controlled, or a steady beam may be modified at any point in its path. The beam may be controlled in many ways. The best and simplest form of apparatus for producing the effect consists of a plane mirror of flexible material—such as silvered mica or microscope glass. Against the back of this mirror the speaker's voice is directed. The light reflected from this mirror is thus thrown into vibrations corresponding to those of the diaphragm itself.

'In arranging the apparatus for the purpose of reproducing sound at a distance, any powerful source of light may be used, but we have experimented chiefly with sunlight. For this purpose a large beam is concentrated by means of a lens upon the diaphragm mirror, and, after reflection, is again rendered parallel by means of another lens. The beam is received at a distant station upon a parabolic reflector, in the focus of which is placed a sensitive selenium cell, connected in a local circuit with a battery and telephone. A large number of trials of this apparatus have been made with the transmitting and receiving instruments so far apart that sounds could not be heard directly through the air. In illustration, I shall describe one of the most recent of these experiments. Mr. Tainter operated the transmitting instrument, which was placed on the top of the Franklin schoolhouse in Washington, and the sensitive receiver was arranged in one of the windows of my laboratory, 1325 L Street, at a distance of 213 metres. Upon placing the telephone to my ear I heard distinctly from the illuminated receiver the words: "Mr. Bell, if you hear what I say, come to the window and wave

your hat." We have found that articulate speech can be reproduced by the oxyhydrogen light, and even by the light of a kerosene lamp. The loudest effects obtained from light are produced by rapidly interrupting the beam by the perforated disc. The great advantage of this form of apparatus for experimental work is the noiselessness of its rotation, admitting the close approach of the receiver without interfering with the audibility of the effect heard from the latter; for it will be understood that musical tones are emitted from the receiver when no sound is made at the transmitter. A silent motion thus produces a sound. In this way musical tones have been heard even from the light of a candle. When distant effects are sought, another apparatus is used. By placing an opaque screen near the rotating disc the beam can be entirely cut off by a slight motion of the hand, and musical signals, like the dots and dashes of the Morse telegraph code, can thus be produced at the distant receiving station.'

The Action of Hollow, as compared with Solid, Steel Magnets, is considered by Herr Holtz in Wiedemann's *Annalen*. He had already come to the conclusion that solid bars do not form good permanent magnets, because the *core*, or central part, absorbs much of the magnetizing force, and because it is equivalent to an armature joining the two poles. Tubes can be forged out of sheet steel, and need not be welded at the junction. A rod and such a tube, 12½ centims. long and 13 millims. in diameter, were compared, the thickness of the tube-wall being 1⅔ millims. They were magnetized to saturation, and the magnetism of the rod was found to bear the ratio to that of the tube of 1 : 1·6. When the rod and tube were 32 centims. in length and 35 millims. in diameter, the difference was as 1 : 1·5. When the tube was filled with a core of soft iron, it hardly retained enough magnetism to obey the directive action of the Earth. After six months of rest, the same magnets were again compared, with the result that in the larger pair the solid magnet retained 1, the hollow 2·5. In the smaller pair, the rod held 1, the tube 2·9. He promises to make further observations on the subject.

An Electro-dynamical paradox is described by M. Gérard-Lescuyer in the *Comptes Rendus* of July. If a current produced by a dynamo-electrical be sent into a magneto-electrical machine, the latter begins to move, but after a time it suddenly slackens, stops, and starts in the opposite direction. This reciprocating motion lasts as long as the producing current. The motive current must evidently change in direction,—a fact proved by introducing a galvanometer into the circuit. This occurs even without change in velocity of the generating machine. If it be supposed that the receiving machine receives a periodical increase of velocity, it would, by virtue of this, give rise to a current of its own, which would traverse the motor instrument in an opposite direction to that emanating from it, and reverse the polarity of the inductors. This is shown to be the case by applying a brake to the receiver sufficient to prevent its increase of velocity: the phenomenon does not then take place. With inductors of cast iron, as in the Gramme machine, the experiment is less certain, owing, as he thinks, to the residual magnetism of the cast-iron, which offers resistance to the reversal of current. Soft iron inductors, on the other hand, permit the action at the first attempt.

The Earth's Rotation was demonstrated by means of the pendulum by Leon Foucault in February 1851. He was permitted to hang a bob of 28

gemmations, which soon give birth to the third larval form, the 'Budding Pseudogyne' (*Pseud. bourgeonnante*). This is apterous, and, except in size, resembles the first larval form. This third form has the power of reproducing its like several times; that is to say, there may be several generations of the third form, but always by gemmation. It was upon this form that the experiments of Bonnet, Kyber, and other old observers were made.

Towards the autumn a fourth larval form appears, and this developes wings. This returns to the *Quercus coccifera*, which is the winter habitation of the species. M. Lichtenstein names it the 'Pupiferous Pseudogyne' (*Pseud. pupifère*), on account of the nature of its products of gemmation. The progeny of all the preceding stages was agamic; that of this fourth form consists of forms which develope into sexually perfect insects. They are of two different sizes, and the insects proceeding from them are also very different. All are apterous and quite mouthless. The smaller ones are males, the larger females, within which may be seen a large egg, filling the whole body of the insect. This is the only true female in the whole series; and after fertilization she deposits the true egg, which in the following spring will give birth to the 'founder' (*fondateur*), which formed our starting-point.

INDEX.

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